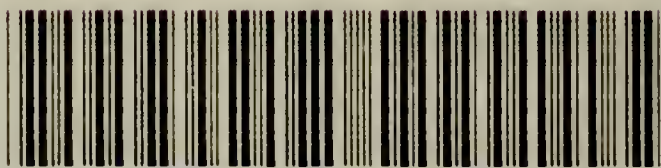


MEDICINE
DISEASE AND DEATH

C. ELAM, M. D.

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MEDICINE, DISEASE,

AND

DEATH;

BEING AN

ENQUIRY INTO THE PROGRESS OF MEDICINE
AS A PRACTICAL ART.

BY

CHARLES ELAM, M.D.,

AUTHOR OF "A PHYSICIAN'S PROBLEMS," &c.



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P R E F A C E.

A "Problem" that has greatly occupied my attention of late years is this :—

*Given : A large and increasing mortality in our population :
To trace the cause and the remedy.*

A careful study of the statistical data bearing on this point has convinced me that there is an increase of aggregate mortality, within the last thirty years, amounting to one in each 1000 persons living. Those who dissent from my conclusions say that the increase is only about half this amount. The most sanguine optimists do not venture to suggest that there is any decrease.

This is a phenomenon well deserving of investigation ; for it cannot be doubted that the immense impulse and development given to sanitary improvements, and to preventive medicine during this period, must have been effectual in saving multitudes of lives. Our better comprehension of the conditions attendant upon the spread of fevers, exanthemata, various forms of blood-poisoning, &c., cannot have been without result in lessening the mortality from these sources. The spread of vaccination has done much to avert the terrors of small-pox. The energetic enquiries made into water supply, and the condition of dwelling-houses, have doubtless tended to an equally beneficial end. Yet is our death-rate increased ; or, to take the most favourable view, not diminished.

To what is this fact attributable? Doubtless in great measure to circumstances connected with our social and industrial organisations; to our still defective sanitary arrangements; to our over-crowded and ill-drained towns and cities; to a systematic neglect of the most ordinary laws of hygiene; in fine, to the headlong rapidity of life generally. All these influences are well known and recognised as demanding the intervention and increased efficiency of "*preventive medicine*," and I have elsewhere discussed them at considerable length. The following pages are devoted to an enquiry, rather suggested than fully investigated, as to how far "*curative medicine*" may possibly contribute to, or fail to counteract, the result in question. Is the medical art advancing *pari passu* with the tributary sciences? Is the Art of Healing, considered in relation to the entire mass of disease, more or less efficient than formerly? Are we to any extent losing sight of the one great object of our profession, the *cure* of disease, in the energy and earnestness with which we cultivate the *knowledge* of it?

Such are the questions involved in the following enquiry; questions not suggested solely by the statistics referred to, but also by phases of thought and action which I have witnessed amongst the profession, and which appear to me to be enormously increasing during the last few years. I refer to a growing indifference to, and want of faith in, therapeutics, properly so called; and an increasing trust in expectancy and stimulation, almost fatalistic in its placidity.

The papers which form the nucleus of this essay have a slight history attached to them, which I only mention as an apology for the irregular and unusual form in which they appear. They were sent to the "*Lancet*" in the early part of this year, and in due time were published. Some time afterwards they were severely criticised in the same journal,—a somewhat unusual proceeding, but one to which I had no objection whatever, as my sole object was to arrive at the truth. At the invitation of the Editor, I sent a reply to these criticisms; which, however, was not considered, when received, to be in suitable form, and, consequently, it did not appear. The astute reader will have no difficulty in divining the true objection to it.

Although the first papers were written with so great a regard

for brevity, and the exigencies of space, as to be meagre and harsh in construction, I have thought it only right to leave them virtually in their original form, so that the applicability of the criticisms, also appended, might be fully apparent. The chapter that follows is only slightly expanded from that which was sent in reply, and rejected.

Since this enquiry was commenced, two essays or discourses have appeared bearing upon the same question, with all the weight of high authority and name. I refer to an essay entitled "The Aims of Modern Medicine," in the "Quarterly Review" for April last,* and to the learned and eloquent "Address in Medicine," delivered before the British Medical Association at the late meeting in Leeds, by Sir William Jenner. Both these authorities are generally supposed and represented to be entirely opposed to my views. And yet, if read without any foregone conclusions, I think they will be found to arrive at virtually the same results, viz., that whilst our "knowledge," our "comprehension," our "appreciation" of disease and its phenomena, are infinitely more accurate and correct than formerly, they have not yet brought forth their legitimate fruits, in the form of *power over* disease. I shall have occasion to enter upon this subject more fully in the sequel.

In one of the critical notices on the "Address" just mentioned, the following passage occurs, obviously directed against the views here propounded:—"If, indeed, there are any who entertain the error eloquently combated by Sir William Jenner, that medicine does not advance as a practical art, they are singularly blind to what is passing in the world around them. Like all the sciences, medicine is taking a wider and more sympathetic scope. Those men who are eminent and devoted in its pursuit are more and more linking its action harmoniously with all the organized development of modern society; and, although the lapse of time while humanity endures may fail to give it all the development of which it is capable, yet we share the hope so ardently expressed by Sir William—that the reporter on progress who shall speak

* This essay appeared early in April, whereas my first paper was not *published* until the 24th of the month. It had, however, been in the printer's hands six weeks before that time.

twenty-five years hence may be able to tell a tale of triumphs in comparison with which the boasted advances of the past shall seem as nothing."

In this hope I cordially and fervently concur ; nay, more, in the fulfilment of this expectation I unfeignedly believe. I am but an enquirer after the truth, and shall sincerely rejoice if it can be demonstrated that the medical art is doing, in the right way, all that can be done, or that it is advancing direct towards such a consummation. But *if there be* any room for improvement, either in our methods of practice or our systems of instruction, I would not rest in the calm conviction that "whatever is, is right" and best ; nor would I be deterred from the enquiry by inflated generalities, or pseudo-eloquent invective.

At the present time I fully believe that there are individual men, in considerable numbers, who, thanks to the progress of medical science, *and observation* with improved aids, are able to treat disease more successfully than at any former time ; but I also believe that owing to the spirit of scepticism now so rife, and defects in our educational methods, the *general result* of medical practice throughout the kingdom is less favourable to life than formerly.

A curious misapprehension of my views seems to have been adopted by some commentators. Because I have suggested the possible necessity for some reforms in our methods and principles, they have represented me as being hostile to the profession and practice of medicine. My critics* accuse me of being influenced by an "*animus*" against medicine ; of making "humbling comparisons," and "writing disparaging things" against my profession, and of being "eager to fasten the blame" upon my own art. This is bad, but worse remains behind. I find I have gained the sympathy and approval of numerous outlaws from, and enemies to, legitimate medicine, who from all sides send me their dreary lucubrations, thinking perchance that I am "altogether such an one as themselves." I wish most earnestly and emphatically to disclaim equally the imputations and the associations. For nearly thirty years all my hopes, prospects, and interests, all my scientific beliefs

* See "Lancet" of July 31st.

and tastes, have been centred in the study and practice of my profession. I have always held, and still hold, a faith in the resources of our art, in the powers and possibilities of medicine, that in these days of scepticism and expectancy would appear quixotic and visionary. It is because of my belief in the high destiny of medicine, properly cultivated, because of my faith in its future resources, that I have ventured to raise the question, whether we are not too much engaged in gathering flowers by the way-side, whilst a rich harvest of fruit is awaiting us, overlooked or neglected?

I recommend this enquiry to the thoughtful consideration of the profession. There were those of old who cried "Peace! Peace!" where there was no peace. Of these I would not be the modern representative, as this was only the watchword of those who had "*healed the hurt of the daughter of my people slightly!*"

18, HARLEY STREET,
December, 1869.

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MEDICINE, DISEASE, AND DEATH.

CHAPTER I.

THE STATISTICS OF MORTALITY.

(From the *Lancet*, April 24th).

THE Registrar-General has recently issued a Report, comparing the deaths in London during the year 1868 with those of the twelve previous years, as to numbers and cause; giving thereby a valuable contribution to the natural history of disease, in the metropolis at least.

It appears to me that a similar investigation, extending over a much longer period, and applying to the whole of England, would be both important and interesting to the profession: and would tend, in some measure, to define our relations, in a curative aspect, to disease, showing whether we are advancing, standing still, or retrograding in the practice of our art.

That the sciences tributary to medicine have been wonderfully developed during the last thirty years is doubted by none. Strange and unexpected as have been the discoveries in general science, they are scarcely greater than those which have revolutionized, and almost re-created medicine. But it has been suspected, and is indeed generally granted, that the therapeutic art has not advanced *pari passu* with our abstract knowledge. The question I would propound, and endeavour to some extent to answer, is as to whether this art has, with regard to the general mass of disease, advanced at all; or whether our weapons of attack and defence are not, as at present wielded, *more powerless* against disease, than they were thirty years ago.

It is impossible in a few words to give even a brief summary of the advances made in the *science* of our profession—that part of it which consists in *knowing*—during the period referred to. Our Pharmacopœia has been perfected, and enriched by the addition of many most valuable agents; and our

Nomenclature of Diseases is perhaps as perfect as professional acumen and scholarship can make it. We have a name and (apparently) a remedy, or at least a palliative, for every disease. It cannot be questioned, also, that the natural history of disease is better understood now, and that some individual affections are more amenable to treatment than formerly. Physiology and pathology have experienced a mighty impulse from the general reception of the doctrine of cell-development; and by this truly grand generalisation the principle of life has been almost compelled to confess its secret. In this branch of research, the microscope and chemistry (a science almost recast during late years) have powerfully aided. Of these matters, as applied to everyday medicine, we knew but little at the period referred to. We had never heard of the endoscope, the ophthalmoscope, the œsophagoscope, the sphygmograph, and a score of other useful aids in diagnosis, which are now in the hands of every student. We had the stethoscope, certainly; but it is scarcely too much to say that at that time the lancet was more frequently in the hand of the general practitioner than the stethoscope. Of the thermometer as a guide in investigation we knew almost nothing. To be brief, nothing can be more satisfactory than the advances we have made in the diagnosis and knowledge of the various ills to which flesh is heir.

But how far can we say of this knowledge that it is power? To know disease, to recognise it, to describe it, to trace its course, its antecedents, and its results—all this is well; but there is something better than this—something to which this is but subsidiary—without which all else is but a dead letter, the dryest of dry bones. Medicine is, or ought to be, primarily and essentially, the Art of Healing, whatever it may be collaterally. If it be not this, it is nothing; and in so far as it departs from this aim, so far it fails to fulfil its great mission, and tends to classify itself amongst the pseudo-sciences.

How far are we as a profession advancing in the fulfilment of this mission? And what, if any, are our shortcomings?

These are questions which ought to be looked fairly in the face,—to be met candidly and answered honestly. I believe we have data whereon to found a true and faithful solution. And when this is furnished, if it should appear that stern facts do not accord with our theories of progress, it will become us not to say carelessly “So much the worse for the

facts," but to inquire into the causes of, and remedies for, our errors; if haply we may yet struggle again into the light.

It has generally been held that, notwithstanding many drawbacks, disease has been brought into better subjection, the public health improved, and the length of the generation increased. I will now proceed to inquire what the inexorable logic of facts teaches us on these heads, and unless I am greatly mistaken we shall find—

1. That the average death-rate is slowly but constantly increasing.

2. That men die now at an earlier average age than they did thirty years back.

3. That even those diseases which are the best understood are increasing progressively in annual mortality, unchecked by any resources of art.

1. Previous to the year 1837 we have no reliable returns of the actual number of deaths. The burial documents of previous years are acknowledged to be imperfect; and miscellaneous writers, however accurate in some respects, are liable to become *loose* in matters of figures. Thus Lord Macaulay intimates that the deaths in an average year, in the reign of Charles II., were 1 in 20 of the population of London—an obvious inaccuracy. But for the last thirty years we have returns of the greatest accuracy, constantly issued by the Registrar-General—documents the value of which it is scarcely possible to over-estimate.

From these we learn, year by year, the average of deaths in every 100 persons living; and, in the vast majority of instances, the causes of such deaths. Year by year these percentages vary: sometimes, in years of epidemics, being considerably above the general average; and then in the year after, or *the year before*, being as much below the calculated number. So that in any one or two years it cannot be pronounced with certainty that the death-rate either increases or diminishes; but, by taking groups of years, the truth may be arrived at, at least approximatively.

Now, in the seven years from 1838 to 1844 inclusive, the average of deaths was 2·189 to each 100 persons living.

In 29 years—from 1838 to 1866—it was 2·242 ditto.

In 7 years—from 1860 to 1866—it was 2·261 ditto.

In 4 years—from 1863 to 1866—it was 2·348 ditto.

The full returns are not yet issued for the last two years ; therefore they are not included here. But these figures sufficiently show a gradual increase in the death-rate.

2. It also appears that of those who die, more proportionately die in childhood and middle life, and in *early* old age, than formerly. Taking the nearest round numbers, we find the deaths from "old age" from 1838 to 1847, vary from 33,000 to 38,000 in the year. After this the numbers fell, and never again rose higher than 29,000, the average being about 26,000 ; and this notwithstanding that the population had increased above 25 per cent. during the period. Perhaps a clearer evidence is obtained from the proportions of deaths to each million of living persons. To each 1,000,000 persons living in 1838, 2408 died of old age ; in 1841, 2389 ; in 1850, 1469 ; in 1857, 1409 ; in 1866, 1361. The obvious answer to this may be, that it is but a difference of registration ; that what used to be called "old age" is now otherwise classed by a better system of nomenclature. I fear this will not serve the cause. A reference to the subjoined table will show that there is an *absolute* falling-off in the number of deaths of very old persons, and a corresponding increase of mortality in the early periods of life. The three years, 1847, 1858, and 1860, are selected for illustration, because 1847 was the last year of a high death-rate amongst the very old, and the other two years are, notwithstanding the great increase of population, very near in absolute mortality to 1847, and therefore more easily contrasted. The results of the other years are nearly identical, but would require more elaborate demonstration. To avoid many figures I will omit the numbers of deaths between 5 years and 45, and enumerate *males* only.

				Deaths of Males in				
				1847.	1858.		1860.	
Under	5 years	...		84·899	...	99·827	...	90·428
From 45 to 55	„	...		14·057	...	14·471	...	14·943
„ 55 to 65	„	...		16·234	...	16·743	...	17·634
„ 65 to 75	„	...		19·092	...	19·433	...	20·327
„ 75 to 85	„	...		15·974	...	15·232	...	15·983
„ 85 to 95	„	...		4·488	...	4·022	...	3·926
Above 95	„	...		·301	...	·230	...	·203

About 12 per cent. should be deducted from the two last columns, on account of the increase of population, to make a proper contrast with 1847. It would then appear clearly that the deaths occurred, on the average, at an earlier age than formerly. Note also that there were 15,000 more deaths in 1858 than in 1847.

3. The history of individual diseases, as traced in the death columns of the Registrar-General, is eminently interesting and curious. With the eruptive fevers and epidemics I will not meddle; the subject would be too complex for my present purpose. Suffice it to say, that there is no evidence of any *decrease* in their fatality. Taking the entire class of “zymotic diseases,” the deaths have increased between 1850 and 1866 from 4409 to 5522 in every million persons living.

Almost all the principal sources of mortality show an increased average of fatality; but there are some which are so marvellous in their increase as almost to pass credence. Of these bronchitis claims the first place.

In the year 1838 (population 15,000,000) there were 2067 deaths registered from bronchitis; in 1847 there were, in round numbers, 16,000; in 1851, 17,000; in 1853, 22,000; in 1855, 27,000; 1858, 29,000; in 1860, 32,000; in 1864, 38,000; in 1866, 41,000. Taking the proportion of deaths to each million of living persons, they rise from 135 in 1838 to 1968 in 1866. In London alone, the deaths from this cause rose *gradually*, between 1840 and 1847, from 500 to 4333. In 1864 they were exactly double—8666!

This enormous increase in figures is *partly* due to a practice, which has been very prevalent of late years, of calling phthisis by the name of bronchitis. In registration connected with private practice there are many obvious reasons for this course, which I need not particularise. In many general hospitals there is a formal rule against the admission of cases of phthisis. The cases *are* admitted, but are called bronchitis. Hence it arises also that phthisis—always far ahead of all other causes of death, and accounting for about one-ninth of the whole number of deaths—does not show the same apparent numerical increase (although there is no material decrease) in the course of years that other orders of disease do. Perhaps the same cause, with a difference, may be supposed to influence the figures of pneumonia, which has perceptibly

increased in fatality, but not to the prodigious extent of bronchitis.

“Heart disease” is another instance of most rapid increase. In 1838 there were 3319 cases registered. In 1850—by gradual yearly increase—the deaths were 10,450; in 1860 they were 17,815; and in 1866 the deaths registered under this head amounted to the almost incredible number of 21,197. In the same period of time the deaths per million persons living have risen from 200 to 1000 from this cause. Difference of registration will again account for a small part of this vast increase, but only a small part. Dropsy and asthma, the diseases most likely to be transferred to “heart disease” under improved registration, show an aggregate diminution in the same periods of about 7 000.

“Brain disease” has very gradually increased from 1407 cases in 1838 to 5605 in 1866; and in the same time paralysis has increased from 4975 deaths to 10,504. “Convulsions” show no marked increase. The fatality of rheumatism has risen from 103 to 115 per million, between 1850 and 1866. Diabetes shows an increase from 24 to 32, and “kidney disease” from 87 to 133, in the same time.

Surgery seems to have been no more successful in averting death than medicine, so far as we can judge. “Joint disease” carried off 52 per million twenty years ago, and 82 per million between 1863-66, each year. “Cancer” shows an increase between 1850 and 1866 from 280 to 395 per million. “Fractures and contusions” increased in fatality from 5000 in 1858 to 6000 in 1866.

All these figures suggest some very grave and serious considerations. If statistics be of any value, then we are compelled to adopt one or other of the following conclusions: Either disease in the aggregate has vastly increased—of which we have no evidence whatever—or the power of medicine in arresting it has materially diminished. In other words, our treatment is less efficient than it was thirty years ago, notwithstanding the vast increase of our absolute knowledge. And this must be true to even a greater extent than appears from the foregoing calculations, for it must be borne in mind that mortality from certain sources has undoubtedly been arrested to a great extent. All our sanitary regulations must have had great influence in lowering what would otherwise

have been the fatality of zymotic disease ; and it is a mere matter of fact that compulsory vaccination has diminished the deaths from small-pox. Other instances might be adduced, each one tending to show, that in proportion as there has been an improvement in certain isolated particulars, so far have we *retrograded in our control over disease in general*. I will endeavour in a future article to trace some of the causes which appear to have effected this result.

CHAPTER II.

ART, SCIENCE, AND EDUCATION.

(From the *Lancet*, June 5th).

IN a former paper (see the *Lancet* of April 24th) I brought forward some statistics, derived from the Annual Reports of the Registrar-General, tending to show—

1. That the annual death-rate is increasing.
2. That the average duration of life is decreasing.
3. That our *power* over disease is in nowise proportionate to our *knowledge* of it ; and that our treatment is less efficient now than it was thirty years ago.

The increase of the death-rate during the period alluded to is, in round numbers, about one in the thousand. This corresponds to three thousand additional deaths in London alone ; and to about twenty-two thousand annually in the whole of England and Wales.

I will not recapitulate further, but proceed from facts to their causes. I believe that for the phenomena immediately in question there are causes in operation sufficiently obvious to be distinctly recognised ; and of such a nature that, if honestly recognised, they may be avoided, and the consequences averted.

I do not think that the medical profession is solely and altogether to blame for the melancholy results on human life above mentioned. There are sources of evil greatly prevalent in society at large, which have been vastly multiplied during late years. Men live much faster than they used to do ; they travel faster ; all the operations of business are conducted much more rapidly than they were ; the wear and tear of life and brain are intensely aggravated. Fortunes are made and

lost with great rapidity; and the hopes, fears, and anxieties attendant upon the transactions are prolific sources of disease and decay. The lists of mortality are thus swelled, without our being able to trace the special causes by the names of the diseases. For great numbers of men die, and the cause of death is called, it may be bronchitis, or pneumonia, or paralysis, or a hundred other names; the real cause being merely the wear and tear, the cares and worries of life.

Then our habits of life are becoming year by year more unnatural. We dine when our forefathers went to bed; and take every precaution to prevent any lapse from an artificial state of existence. Hence new and varied forms of indigestion, with its thousand sons.

Doubtless these and many other circumstances connected with our present social condition do, to some extent, influence unfavourably our vital statistics. But we cannot attribute any *considerable* proportion of the increased mortality to these causes. It is, of course, difficult to estimate with exactness the precise share each cause has in the general result. But if we may assume, as seems most likely, that the male sex will be chiefly affected by the influences now alluded to, we shall be able to arrive, by a process not necessary to dwell upon, at the conclusion, that not more than one-tenth part of the increased mortality is attributable to changes consequent upon the constitution and development of society, and social life in general.

Yet the fact remains, that about 23 persons out of every 1000 die annually at the present time; whereas the average, twenty-five or thirty years ago, was rather under 22 in the 1000. I believe that this is in some measure at least attributable to the neglect or decline of medicine as an art, and, consequently, our diminished power in checking or controlling disease.

This will, doubtless, appear paradoxical, or something worse, to the minds of many. It seems strange enough to speak of the "Decline of Medicine" at a time when medical science is cultivated with more method, persistence, zeal, skill, and *success* than at any former period of our history. That all this is so I most firmly believe, and give all honour to those who give up their lives to these investigations. Yet I equally believe that, as a profession, we have gone astray from our true

mission. It is to some extent true now, as it was in the day when Bacon wrote, "that Medicine is a science which hath been more professed than laboured, more laboured than advanced; the labour being, in my judgment, more in a circle than progressive. I find much iteration, but small addition."

The field for thought that is here opened is very extensive: it may be, boundless. My object is to direct attention to a few only of our most obvious errors, and those that are most fruitful for evil. These may be noticed under four heads: as errors of theory, of education, of practice, and of fashion.

1. I have stated before, that Medicine should be, before all things, the *art of healing*. I believe that our first and fundamental error in theory has been the overlooking of this definition, and viewing Medicine as a science. Now, in any proper acceptation of words, Medicine is not, and can never be, a science; it is an art, *sui generis*—fed, fortified, and enlightened by science,—but in nowise a science in itself. This is not a mere speculative matter—an affair of terminology. So long as we view Medicine as a science, so long shall we be disappointed in our progress and our results. Science is steady, certain, and progressive; art is vacillating, doubtful, and limited. If we expect exactitude and certainty in Medicine, because we rank it as a science, we shall be ever failing, ever doubting and losing our faith, and, as a necessary consequence, our zeal. We can never hope to reduce Medicine to mathematical formulas, in which disease and remedy will represent one side of the equation, and health the other. We deal with such an infinite variety of unknown quantities, and indefinite variables, that we can never hope to reduce them to any fixed expression that can have a practical value. Besides disease, we have to deal in each case with age, sex, temperament, and previous history—elements which can to some extent be allowed for; we have also to deal with idiosyncrasy, with heritage, and with the thousand social surroundings, which will always baffle our calculations, and stultify our foregone conclusions.

At various epochs in the history of Medicine, it has been hoped that it might become a special branch of chemical science. Some years ago this hope assumed a more definite and apparently promising form than it had ever before done. How we have been disappointed in this direction it is needless to show. No one now contemplates the possibility—save in

some few exceptional instances—of forming out of two active disturbing causes, disease and chemical remedy, the neutral compound, health. Because there is a prevalence of acidity superficially apparent, we do not *therefore* calculate that alkalis will cure the pathological condition in which it is manifested, nor *vice versa*. We cannot with certainty calculate even upon altering an acid or an alkaline condition of the secretions by the obvious *chemical* resources. On the contrary, there is no more certain mode of relief known, for some conditions of the stomach in which intense acidity is a prominent symptom, than the administration of acids.

Equally futile in failure and disappointment has been every attempt to derive *methods* of healing from scientific *principles* of physiology or pathology, or to attach them to the formulas of any exact science whatever.

Science is knowledge, but such knowledge in Medicine is not power in any practical sense. We know the motions of the planets, and can predict their phenomena with the utmost exactness, but we cannot influence them in any way. By science we know disease: science is diagnostic. It is by art that we treat it: art is therapeutic. All our art is derived from experience. It may be that in some few instances *à priori* considerations lead us to try certain modes of treatment; but in general they are empirical, and in all cases the final acceptance or rejection of the method is governed by experience. This could not be so were Medicine a science. Science knows, and is precise and positive. Art is variable, and selects. Science submits to no ignorance; but art is ignorant of much. Science is essentially contemplative; art is active. In the apt antithesis of Dr. John Brown: Science puffeth up; art buildeth up.

Practically, the result of this error of theory is this; with every advance of science, we are too much disposed to think that an alteration in our art is necessary; otherwise we should be tacitly admitting the barrenness of the science. We forget the results of long experience, to run after the phantoms evoked by our improved knowledge. We make a discovery in chemistry or in microscopical science, and we are but dissatisfied if we cannot adapt it to our art. We improve in physiological knowledge; we learn the functions of a nervous tract with greater certainty; or we trace the relations of certain

organs to extraneous influence more accurately ; and in accordance with this, we alter modes of treatment which, up to the present time, we have been accustomed to think and to find satisfactory. Our disappointment in the result does not always teach us wisdom for the future.

I do not propose to enter deeply into the abstract question, but will merely state what I believe to be the fact, that pure science has in general done but little for art ; whilst art has constantly and largely been contributing to the progress of science. In our profession this has eminently been the case : not the men of science, but those of careful and accurate observation, have generally been the men distinguished for healing gifts. Avoiding any allusions to men of the present day, let me illustrate my meaning by contrasting Harvey, the man of science, with Sydenham, the man of concrete observation ; Sir Charles Bell, the discoverer, with Abercrombie, the physician.

Perhaps the most important and significant distinction between Science and Art is, that Science can be taught and transmitted, entire and perfect, whilst Art can only be acquired with more or less imperfection, according to the capacities and aptitudes of the recipient. Art is essentially personal ; science is general or universal.

Medicine has the same relation to science that poetry or painting has ; and inasmuch as the most complete knowledge of the laws of perspective and the theory of light and colours would fail to make a painter ; or the most intimate acquaintance with the rules of versification would fail to make a poet ; so the profoundest knowledge of physiology and of all the sciences tributary to Medicine would entirely fail to make a competent physician. Medicine is a *faculty* to be acquired, not a lesson to be learned ;—to be acquired by long and patient observation of complex phenomena, in their ever-varying combinations,—not to be reduced to the hard and inelastic formulæ of science. In itself, I reverence science ; but, in the interests of true progress and of humanity, I trust we shall, for the future, hear more of the “ art of healing ” and less of the “ science ” of Medicine.

2. The most important consequence of this theoretical error is, the false system of education that is entailed and necessitated thereby. Medicine being considered a science, it appears

necessary to cultivate all the collateral sciences to the utmost. A man may be legally qualified to practise medicine at twenty-one years of age. By that time he must have an accurate and minute knowledge of descriptive and general Anatomy; he must be well versed in the latest views on Physiology; his knowledge of Pathology must be well vouched for; he must be sufficiently versed in Chemistry to be enabled to analyse ordinary solutions, besides cumbering his memory with names, numbers, and proportions without limit; he must be able to describe in scientific phrase any plant which enters into the composition of the *Materia Medica*, and to anatomise any leaf or flower *secundum artem*; and he must have a competent theoretical knowledge of the principles and practice of Medicine and Surgery. By this time, also, he will have seen much operative surgery, and a minimum of medical practice at the hospital; and he must have practised vaccination, obstetrics, and the minor operations of surgery; eking out this trifling curriculum with the study of Forensic Medicine and Toxicology. Should he be destined for the higher walks of the profession, it would be much easier and shorter to enumerate the few subjects he need not study, than to catalogue the heterogeneous farrago of learning with which he must bewilder his unfortunate brain. It is evident that much of this learning must be acquired in a superficial and perfunctory manner, and that the knowledge necessary for passing the examinations must be "crammed"—to be disgorged and forgotten at the earliest possible period afterwards.

Against the combined wisdom of modern authorities, I would not venture to assert positively that all this is not wise or necessary. It may be that a precise knowledge of all the processes and foramina of the sphenoid and other bones is an urgent matter; that we cannot give quinine with due effect unless we can describe botanically the *Cinchona oblongifolia*; and that we are virtually disqualified from administering a dose of calomel, unless we remember accurately its chemical composition and mode of preparation. But this I can assert—that numbers of young men, hard-working, conscientious students, gifted with intelligence above the average, who have passed through the schools with a single eye to their ultimate mission, with credit and distinction, have confessed to me with bitterness and shame how utterly helpless they felt when

brought face to face with disease, and how intolerably far apart from the great purpose of their lives was almost the whole of their former studies. A great French surgeon once spoke of the necessity of "spoiling a hatful of eyes" in learning the extraction of cataract: how many hecatombs of patients are likely to perish whilst the student is painfully forgetting his school lore, and slowly acquiring the art of healing? This art must be learned by practice, accompanied by practical tuition; and a very large proportion of all else that is now taught is just as necessary, as it would be to insist that a painter must be able to make his own colours, and be acquainted with their natural history and chemical constitution, before he is allowed to exhibit a picture.

That a physician should be a man of science is a very desirable thing; but he ought *first* to be a physician, and accessorially the other; whereas, under our present system of education, he is first made a man of superficial science, and is left to take the remote and uncertain chance of becoming a physician, in the true sense of the word.

I believe that here is the root of the evil, however difficult it may be to devise a remedy. The ranks of our profession are constantly recruited with young, *very* young men, who confess themselves, in a great many instances, incompetent to grapple with disease;—well educated, highly cultivated, it may be, in science, but wholly helpless when confronted with the responsibility of a serious case. Feeling their own inefficiency, they are prepared to adopt any views or theories of practice, however wild, that are propounded with sufficient confidence, or that are the fashion of the day. Science has *principles*, whereas art has *methods*. Instead of searching after methods, the tendency of the day is to seek for principles. As these cannot, by the hypothesis, be attained, loose and plausible theories are adopted in their place. Hence the variable and vacillating character of our treatment, and the fatal error ultimately, of losing faith in all therapeutics, except expectancy and *stimulation*.

3 and 4. This brings me to the errors of fashion and practice, which it is more convenient to discuss together. To enter fully into this subject would be to write the history of Medicine. For this I am not prepared. I can, therefore, but give a few illustrations of my meaning.

I believe that, in deference to popular prejudices and pseudo-scientific theories, we have from time to time forsaken and lost sight of modes of treatment that had been tried and proved as efficacious by long years of experience. The murrain of homœopathy and other follies came upon us for our sins. Many of our profession having, from causes already stated, no reliable methods of their own, adopted the new lights, partly from fashion, partly from lack of any convictions whatever, and partly from failure in wielding the legitimate weapons of war against disease. Many others, professing eclectic impartiality, neither adopted these views nor adhered to their own, but lapsed into utter unbelief as to our power of influencing disease. Others, again, witnessing the conflict of opinions amongst intelligent men, grew utterly careless, thinking that any course of treatment would do well that was the least irksome. Finally, a large majority forsook certain forms of *active* treatment, such as had been proved to be powerful in controlling certain diseases, because they seemed to be no longer in accordance “with the spirit of the age;” and attempted to justify this to their own minds by some theories of “change of type” in disease.

An apt illustration of these remarks may be drawn from bleeding. I am no advocate for the indiscriminate use of the lancet that prevailed at one time; but I believe that in forsaking it so entirely as we have done, we have relinquished a most powerful and efficient agent in therapeutics. It was in former days the practice to treat many inflammatory and congestive affections by bleeding; and those who remember the results cannot fail to see that many cases were cured or cut short by this course, that are now left to the chances of almost spontaneous cure; certainly with unfavourable results to mortality. We seem to have forgotten that spontaneous hæmorrhage, from the nose or elsewhere, often appears to avert serious illness, and to indicate the artificial mode of cure. Speaking from personal remembrance, no theory can ever make me forget the blissful, immediate, and perfect relief that twice followed bleeding in two attacks of croup. I have, even in modern times, also seen the most marvellous alteration for the better produced by bloodletting in cases of lung-congestion resulting from heart-disease, when the “supporting system” had been long persevered in, with the sole effect of aggravating

the symptoms. Is it not worthy of consideration, also, whether the increased fatality of apoplexy, and sundry other affections of the brain, is not in some measure due to our neglect of this agency?

In like manner has the use of mercurial preparations been subjected to the vagaries of fashion and theory. Until late years, the utility of mercury in some affections of the liver, for instance, was never doubted, and the results of practice were strongly confirmatory of this view. Yet no sooner is it discovered that mercury does something, or fails to do something, in the animal economy, that was not known before, than we ignore our previous therapeutic experience and adopt some other untried method of cure.*

The same observations may apply with equal force to the use of active purgatives and other potent agents of our *materia medica*. We now make but little use of these, compared with old times. And yet no one who has been much engaged in actual practice can fail to remember numerous instances in which great and immediate benefit has accrued, obviously *from* their use. Does not this prove that our disappointment in the general results, and our consequent abandonment of the means, are due simply to our own ignorance,—to the lack of careful, persistent observation of symptoms, and in nowise to want of power or virtue in the remedies themselves?

I have reserved for this late consideration one monstrous and gigantic source of evil—a compound of theory and resultant practice,—which I believe to be the cause of more avoidable deaths than all our other errors combined. The theory is, that all disease tends to death, and *therefore* the powers of life must be supported. The practice is, the great prevalence of feeding and excessive stimulation. Space does not allow me to enter upon any scientific investigation of the subject. I must content myself with giving the *result* of long and careful thought, with ample test and experiment in hospital practice.

* It is not long since that I saw a case of simple jaundice that had been under the care of one of our most eminent specialists. It had resisted the most *strenuous efforts* to cure it by small doses of solution of acetate of ammonia (!); and this being the case, the patient was told that it was a case of “scirrhus of the liver,” and was sent home to die! No active treatment, and no mercurial, had been tried. A few small doses of mercury-with-chalk, with other mild treatment, removed the affection; and I believe the patient is alive and well at the present time.

That result is, that nothing can be more erroneous than the theory—nothing more fatal or more fruitful in proteiform evil than the practice.

I have stated in the previous chapter that the deaths from bronchitis have increased from 2067 in 1838 to 41,000 in 1866, and that those from “heart disease” have increased during the same time from 3319 to 21,197. Now bronchitis is not, or rather ought not to be, an essentially fatal disease, in the sense that cancer, pyæmia, or phthisis may be considered such. Disease of the heart is likely, in some degree, to shorten life, but is by no means so essentially and speedily fatal as is often supposed. It is almost always by the production of secondary and congestive affections that disease of the heart proves fatal, and if these can be warded off, life may be prolonged indefinitely. I have known many patients with valvular obstruction live a long term of years, and at the end be hurried off by indiscreet zeal in treatment. I cannot give individual instances for obvious reasons.*

With regard to these two diseases, bronchitis and heart affection, the increased mortality in which is so enormous as to account for the whole average increase in deaths, I have not the slightest hesitation in attributing the fatal result, in a vast proportion of the cases, to the vicious system of treatment that has become more and more prevalent for many years, and especially to that most pernicious habit or fashion of giving stimulants largely and indiscriminately. Alcohol is poison in bronchitis, speaking generally; and in affections of the heart there is nothing that so much favours the development of local congestions as these stimulants.

Another serious evil connected with this practice is its ultimate result on individuals and families, apart from the disease. By the loose method in which many of our profession order wine and brandy for even slight neuralgic affections, a taste for drinking is established, the consequences of which are often not to be calculated. Most assuredly I have seen large families swept off entire, all by affections connected with alcoholism, the original use of the stimulant^{*} having been “by medical order.”

I wish very briefly to notice two other circumstances connected with our modes of practice; one of which has a tendency to render therapeutics uncertain; the other to hinder

* One illustration of my meaning is given in chap. vii.

the true advance of Medicine. I refer to the constant running after new medical agencies ; and to the system of medical specialisms. The evils of the former practice are patent and glaring. New drugs are introduced, and vaunted as specifics ; and before their virtues or properties have been ascertained by experience, they are deposed in popular favour by some other and newer remedy. Hence arises our woful want of true knowledge as to the weapons at our command, their virtues, and the proper method of using them.

I cannot venture to say much upon the practice, now so prevalent, of dismembering the organism, and making a specialism of the treatment of some one class of diseases.* I believe that the true method of viewing disease is, as a *departure from proper co-ordination of all the functions*. If this be so, then the prevalent practice must be opposed to the advance of true knowledge ; not to dwell upon the fact that thereby our best men are withdrawn from the field of general utility, and the great and all-important subject, the detection and treatment of *obscure disease*, is systematically neglected. But with this brief hint I dismiss the subject at present, as being too extensive, and, I may add, too delicate in relation to the prejudices of many, to dwell upon more fully.

Thus I have endeavoured, very imperfectly, to trace a few of the most prominent causes for the sad phenomenon of the retrogression of our art, *pari passu* with the advance of our science. The knowledge of this evil should suggest the remedy, so urgently needed. Perhaps some bolder pen than mine may take up the theme, and expose the “sores and imposthumes” from which we, as a profession, are suffering. Meanwhile, I would only suggest that our first reform should be directed to our systems of education,—not to improve upon those now in vogue, but to begin again *ab initio*,—to devise some real, sound, practical system of teaching and learning the Art of Healing.

* I refer here wholly and solely to specialism in *medical* disease. I believe it to be a necessary and advisable proceeding in many departments of *surgery*.

CHAPTER III.

“ AUDI ALTERAM PARTEM.”

(From the *Lancet*, July 10th and 17th.)

“It will hardly be denied that in the papers which have appeared in this journal* under the above heading, Dr. Elam has raised points of very considerable interest to the profession. Starting with the general principle that ‘Medicine is, or ought to be, primarily and essentially, the Art of Healing, whatever it may be collaterally,’ he has been led—mainly, as it would seem, by an examination of death-rates—to the conclusion that the power of medicine in checking or controlling disease has diminished in this country. He has, in short, formulated three distinct propositions—namely: 1. That the average death-rate is slowly but constantly increasing. 2. That men die now at an earlier average age than they did thirty years back. 3. That even those diseases which are the best understood are increasing progressively in annual mortality, unchecked by any resources of art. And in developing his ideas upon these somewhat momentous questions, Dr. Elam makes use of two lines of argument; one based entirely upon statistical inferences, and the other drawn from his own practical experience and knowledge. To the latter of these we shall address ourselves on a future occasion; taking this opportunity—the earliest that has occurred—of deprecating what appears to us to be the sweeping and exaggerated conclusion that, even supposing the death-rate of the whole aggregate population to have increased of late years, it would necessarily follow that our treatment of disease is less efficient than it was. We cannot help thinking that, not only in the profession itself, but among those persons outside its ranks who have had any experience of statistical analysis and the fallacies frequently met with in deductions therefrom, there will be many inclined to ask with us whether the statistics upon which Dr. Elam relies do really supply ‘the inexorable logic of facts’ assumed by him; or whether, on the contrary, he has not been led astray by the semblance of truth which figures are often apt to wear.

“Now we may say at once that we have little sympathy with the mistrust of statistics which with some minds amounts to a perfect craze. You may, in a sense, ‘prove anything by figures,’ just as you may by a perversion of logic prove that black is white; but the true recipe for using statistics is the same as Opie’s recipe for mixing colours—‘With brains, sir;’ and used in that wise their value is undoubted.

“Let us see, first, then, how far it is true that the average death

* Vol. i. 1869, pp. 560, 775, and 809.

rate is ‘constantly increasing.’ Dr. Elam, on looking at the Registrar-General’s mortality tables, observed that ‘year by year these percentages vary,’ in consequence of epidemic disturbances, ‘so that in any one or two years it cannot be pronounced with certainty that the death-rate either increases or diminishes’—an admission the force of which he appears to have subsequently forgotten. To arrive at the truth, therefore, he threw the years into groups thus:—

29 years—1838-66	22·42	per 1000.
7 years—1838-44	21·89	„
7 years—1860-66	22·61	„
4 years—1863-66	23·48	„

From these results it is made to appear that the present death-rate is about 1 per thousand in excess of what it was thirty years ago. That our readers may form their own opinion upon this matter, we extract from the Thirtieth Annual Report of the Registrar-General the following death-rates for the English population during each of the years since registration came first into operation, as well as the rate of birth, which is an element to be considered:—

Years ended Dec. 31st.	To 1000 persons living.		Years ended Dec. 31st.	To 1000 persons living.	
	Births.	Deaths.		Births.	Deaths.
1838	30·29	22·38	1854	34·08	23·52
1839	31·75	21·85	1855	33·73	22·61
1840	31·95	22·88	1856	34·53	20·51
1841	32·15	21·59	1857	34·43	21·80
1842	32·11	21·68	1858	33·66	23·09
1843	32·31	21·23	1859	35·04	22·39
1844	32·73	21·61	1860	34·37	21·24
1845	32·51	20·89	1861	34·61	21·63
1846	33·83	23·06	1862	35·04	21·47
1847	31·52	24·71	1863	35·39	23·05
1848	32·47	23·06	1864	35·64	23·86
1849	32·94	25·12	1865	35·64	23·39
1850	33·40	20·77	1866	35·54	23·61
1851	34·25	21·99	1867	35·85	21·98
1852	34·30	22·38		—	—
1853	33·27	22·88	Mean	33·64	22·41

“It is plain upon the face of it that, in grouping several years together, the result will be materially affected by the principle of selection adopted. Setting aside the rates for the first three years, 1838-40, when registration was in its infancy, and adopting Dr. Elam’s reason for throwing the years into groups, we subjoin the mean annual rates of birth and death per 1000 of population, in equal periods of five years, starting from the Census year 1841:—

	Birth-rate.	Death-rate.		Birth-rate.	Death-rate.
1841-45	32·36	21·40	1856-60	34·41	21·81
1846-50	32·83	23·34	1861-65	35·46	22·68
1851-55	33·93	22·68			

The mean rate for the two odd years 1866-67 is, of births 35·70, and of deaths 22·80. With a reservation in reference to the value for purposes of *exact* comparison of death-rates for years since 1861, calculated on an estimated increase of population, it appears that, while the ratio of births has steadily and constantly increased, the quinquennial ratio of deaths has fluctuated more or less both in the direction of increase and decline. In the two decenniums 1841-50 and 1851-60, the mean death-rate decreased from 22·37 to 22·24 ; in the twenty years 1841-60 the rate was 22·30 ; in the seven years (1861-67) which have elapsed since the census was last taken, the mean death-rate (assuming the population to be correctly estimated) was 22·74 : the net result being that, with an admitted source of uncertainty since 1861 in one of the factors employed, the increase in the ratio of deaths does not amount to ·5 per 1000, while the ratio of births has increased to the extent of 2·20 per 1000.

“ The truth is, that the mere average death-rate of the aggregate population of a country, without regard to a variety of conditions fluctuating within wide and comparatively unknown limits, although useful enough for purposes of general approximation in respect of health and disease, cannot seriously be held to give anything like the precise and definite measure of the progress of medicine as a healing art which Dr. Elam has supposed. The degree in which the laws of mortality are affected by the treatment of disease is small by comparison with the influence exercised by social and sanitary conditions, by weather, and by a number of other conditions, very many of which Dr. Elam has himself particularised, but for which he has made, we think, most inadequate allowance. The elaborate Report of Dr. Farr on the mortality in the different registration districts of England in the ten years 1851-60* shows how the death-rate varies under different circumstances, among different classes, and in different localities ; and the great lesson which appears to us to be taught by Dr. Farr in that Report is that death-rates which do not take those variable elements into account are mere rough and ready indications, useful if cautiously handled, but of no exact scientific value, and certainly untrustworthy as a means for gauging the progress of medical treatment.

“ The striking difference between the general death-rate in towns as compared with country districts, illustrates very clearly a weak point in Dr. Elam’s reasoning. There is a difference of between 4 and 5 deaths per 1000 in the mortality of the population thrown roughly into the two classes of urban and rural residents ; and, moreover, it is noticeable that it is amongst the town populations that the greatest fluctuations of mortality occur. And naturally so, for the population increases four times as fast in the towns as in the country districts ; the birth-rate is very much higher, and the general departure from the characteristics of mere normal life

* Supplement to 25th Report of Registrar-General.

contingencies is, of course, far greater also in the towns. How far is it reasonable to ascribe even the small increase of mortality assumed by Dr. Elam to any falling off in the efficacy of medical treatment without at the same time showing that the conditions under which disease is treated have not deteriorated also? What force of argument do death-rates supply in support of Dr. Elam's assertions when it is seen that, taking the 640 registration districts of England, the mortality in the two decenniums—1841-50 and 1851-60—declined in 220 districts, increased in 210 districts, and remained virtually stationary in the remaining 210 districts? Will anybody infer from this that the art of healing has progressed in those 220 districts, retrograded in those 210 districts, and remained *in statu quo* in the remainder? Surely not; although in that case what becomes of Dr. Elam's argument? There are towns, like Salisbury, where the mortality has been reduced as much as 7 per 1000; but we, at any rate, should not venture to claim for medical skill in Salisbury that it has made such rapid progress as to account for so great a reduction in the mortality.

“But we may quote further, in opposition to Dr. Elam, the following sentences of Dr. Farr in his last Report to the Registrar-General on the Causes of Death in England. After referring to the circumstances under which the suppression of a fatal type of disease did not diminish the mortality in a northern city many years ago, Dr. Farr says: ‘It is under unfavourable conditions of the same kind, although less in degree, that the mortality is now sustained in England, where the town populations, constantly increasing, without equivalent arrangements for drainage and for accommodation in dwellings, are every year exposed to increasing dangers It is impossible, in the present state of science, to reduce under any simple law the phenomena of disease development; but disease development is evidently associated with the life development of species, and has with it some analogies . . . The rate of death is, under the same conditions over a series of years, nearly constant.’

“Dr. Elam holds that, if statistics be of any value, we are compelled to adopt one or other of these conclusions: either disease in the aggregate has vastly increased—‘of which,’ he says, ‘we have no evidence whatever;’ or the power of medicine in arresting it has materially diminished. Now about the increased or diminished prevalence of disease we have literally no facts, no statistics whatever, to guide us, in reference to the aggregate population of this country; but we have a strong impression that it will be found, on examination of records kept here and there in particular localities, that whenever the mortality has increased, disease has been unusually prevalent to an extent more than sufficient, or at any rate quite enough, to account for the rise in the death-rate.* It is,

* Since the above was written, Dr. Ballard's Annual Report on the Health of the Parish of St. Mary, Islington, during the year 1868, has come to hand; and

however, absolutely certain that whether or not disease is on the whole more fatal now than it used to be, neither Dr. Elam nor anybody else has any reliable means of knowing. We must wait until we have had a system of disease registration in force throughout the country, side by side with the registration of deaths, say for half a score years, before anybody can possibly be justified in saying that disease is more fatal than it used to be ; and even then, before any charge of retrogression can be brought home to the profession in respect of disease treatment, it must be proved that the proportion of cases of disease brought under treatment has kept pace with the growth of population and with the increased susceptibility to disease resulting from unhealthy conditions of life.

“ Upon Dr. Elam’s second proposition we have need to say very little, beyond expressing our surprise that so able a writer should have laid the slightest stress upon so weak an illustration as is afforded by the deaths ascribed in the registers to ‘old age’ in support of the assertion that men die now at an earlier average age than they did thirty years ago. We have no evidence that Dr. Elam has gone into the elaborate calculations, which alone would warrant him in speaking so positively upon this head ; on the contrary, he appears to have relied solely upon the fact that in certain *selected* years the proportion of deaths among young children to the total deaths exhibited an increase, while among old people the proportion declined. No proper allowance is made for increase of population : to make the deaths at different ages in two of the selected years, 1858 and 1860, comparable with those in the other selected year, 1847, we are told that ‘about 12 per cent. should be deducted on account of the increase of population ;’ clearly implying a uniform rate of increase among persons through every group of ages, although our best statisticians have demonstrated over and over again that the young, middle-aged, and old show a very wide difference in their observed rates of increase at periodical census enumerations. It is much to be regretted that, prior to any expo-

as this very able health officer has for some years kept records of the cases of sickness arising in public practice within his parish, we may mention two or three facts taken from his Report which bear out what we have said in regard to the relation between greater or less prevalence of disease and a rise or fall in the death-rate. Thus in 1867 the public medical officers treated 34,692 new cases of disease, whereas in 1868 they were called upon to treat 40,302 of such new cases ; the general death-rate of the parish being 19·9 per 1000 in the former and 20·7 per 1000 in the latter year. That is to say, the cases of sickness increased 16 per cent. in the year, and the deaths 9 per cent. Making full allowance for the fact that the deaths occurred amongst all classes of the Islington population, whereas the sickness only of the poor is taken into consideration, it yet appears to us that the facts do show an increased amount of disease which would more than account for the rise in the mortality of the parish. Dr. Philipson’s Sickness Returns for Newcastle, those of the Manchester and Salford Sanitary Association, and of other health officers in this country, may also be appealed to in disproof of Dr. Elam’s statement that disease has not increased.

sition of his views about the testimony of statistics to the shortcomings of medicine, Dr. Elam should not have put himself through a course of study of the Reports of our chief vital statist, and so have avoided the palpable errors into which he has fallen. He has, for example, in the point now under consideration, relied exclusively upon the *mean age at death*, in stating that the deaths occurred (in latter years), on the average, at an earlier age than formerly; whereas, in the Registrar-General's Fifth Report, and on many subsequent occasions, Dr. Farr has told us that the mean age at death is utterly fallacious as a test of the value of life in the abstract, much less of general or relative salubrity, among a population constituted as is that of this country. The deaths at different ages in given years must be considered relatively to the ages of the population out of which they occurred, if the true rate of mortality is required; and this Dr. Elam has omitted to take into consideration. The Census Returns of 1861 show that the average age of the whole population in 1851 was 26·4 years, and 26·5 years in 1861; and it would surely be as unreasonable to attribute this slight increase in the mean age of the living to improved medical treatment, as it is to ascribe a slight decrease in the mean age at death to the decline of the healing art.

“Without admitting for a moment that rates of mortality give any necessary or reliable indication of disease treatment, we nevertheless subjoin the rates per 1000 of males at different ages relatively to the population in each of the three Census years—1841, 1851, and 1861; taking those years for the simple reason that they are the only ones in which we have any absolute knowledge of the distribution of ages amongst the living.

		1841.		1851.		1861.
All ages	...	22·38	...	22·76	...	22·68
0—5	...	68·43	...	72·98	...	71·76
5—10	...	9·56	...	8·69	...	6·74
10—15	...	5·10	...	4·91	...	4·33
15—25	...	8·11	...	7·76	...	7·28
25—35	...	9·78	...	9·48	...	9·23
35—45	...	12·17	...	12·36	...	12·65
45—55	...	17·85	...	17·87	...	16·90
55—65	...	31·37	...	30·31	...	30·08
65—75	...	64·82	...	63·96	...	68·90
75—85	...	142·66	...	140·55	...	146·54
85—95	...	296·50	...	282·45	...	310·92
95 & upwards		431·64	...	419·37	...	448·35

These authentic statements of fact tell their own story. We may fairly point out, however, that, according to Dr. Elam's own reasoning, we may claim for Medicine, that although she may not have succeeded in reducing the mortality of infants and of sexagenarians, yet she appears to have had a triumph among the great bulk of the population between 5 and 60 years of age.

“ We must dismiss with few words Dr. Elam’s endeavour to trace the effects of unsuccessful treatment in the increased mortality from certain diseases. He takes bronchitis as an example. He says that the proportion of deaths to each million of population rose from 135 in 1838 to 1968 in 1866 ; and gives the absolute deaths from this cause in certain years so selected as to make it appear that the increase was steady and continuous. With a reservation against being supposed to admit the force of any argument drawn from the death-ratio of a disease so liable to be confused in the returns with other diseases, we give the mortality per million from bronchitis successively for each year, beginning with 1851 down to 1867 inclusive :—978, 953, 1237, 1092, 1467, 1144, 1341, 1509, 1332, 1648, 1558, 1617, 1574, 1894, 1754, 1968, 1902. We have no desire to use a harsh expression ; but really it is too absurd to base any serious argument about the increased inefficacy of medicine upon so unsubstantial a foundation as is supplied by the fluctuating fatality of a disease dependent upon atmospheric and other conditions for its greater or less prevalence in successive years. Will any rational being believe, as Dr. Elam implies, that there has been no perceptible difference in the number of attacks of bronchitis occurring in these years ? The mortality was less in 1867 than in 1866 from this cause. Is that result attributable to the increased efficacy of medical treatment of bronchitis ? Is it not, in all probability, due rather to the fact that there were fewer persons attacked with bronchitis in 1867 than in 1866 ?

“ And here we must bring our examination of Dr. Elam’s statistics to a close, having devoted so much space to their consideration out of respect for an esteemed and very able member of our profession, whose position might otherwise have led many readers to take his statements as expositions of a new and very startling discovery. We have taken pains to test almost every figure used by Dr. Elam by the light of official documents, and the result has been thoroughly and completely to satisfy us of their utter failure to establish either one or other of his propositions relative to the decline of medicine as a healing art.”

CHAPTER IV.

THE LEADING ARTICLE FROM THE “ LANCET ” OF JULY 31ST.

“ The subjects of Dr. Elam’s papers, ‘ Medicine, Disease, and Death,’ are too interesting for the papers not to have been extensively read. The conclusions to which Dr. Elam comes are too serious to be accepted without reluctance, and, unless we are greatly mistaken, are very erroneous. It would be discouraging indeed if, with the enormous increase of our knowledge of late years, our art

had really retrogressed. However, the question is not whether Dr. Elam's conclusions are flattering and satisfactory to the profession, but whether they are right—whether we are really doing less for the mitigation of suffering and the postponement of death than our forefathers did. It is very easy to put together three great vague words, such as give the attractive title to Dr. Elam's papers, and, with the help of statistics, come to almost any conclusion on the subject to which the mood or fancy of the writer may incline him. But it is very doubtful if sound conclusions are to be easily reached in this way. Without determining here the value of the statistical method of judging of medicine as an art, we may say that the data are not yet in existence upon which such a judgment can properly be based. The art of diagnosis is still too imperfect, and the returns of the Registrar-General are still composed of too vague material, to allow us to think that in comparing the same names we are comparing cases really the same, and having the same significance. The late Dr. Alison, of Edinburgh, was wont to make a remark which we commend to Dr. Elam, to the effect that one carefully observed fact or case in medicine was worth more for practical purposes than a great mass of figures.

“Before criticising more particularly Dr. Elam's conclusions, let us be clear as to the nature of them. He thinks—1st, that the average death-rate is slowly but constantly increasing; 2nd, that men die now at an earlier average age than they did thirty years back; 3rd, that even those diseases which are the best understood are increasing progressively in annual mortality, unchecked by any resources of art. Dr. Elam thinks that either disease has increased, of which, he says, we have no evidence, or the power of medicine in arresting it has diminished, because the principal sources of mortality show an increased average of fatality.

“Now, we are not going to revert to the statistical view of Dr. Elam's papers. We thought it right to examine the statistics of the subject, and we think we have shown that, from the merely statistical point of view, Dr. Elam's conclusions are very unwarranted, not to say absurd. We have shown that the death-rate fluctuates wonderfully from year to year, and varies in different districts; that these fluctuations and variations are of such a nature as to be quite inexplicable by a mere reference to the state of medical art; that the death-rate in the last decennium was less than in the previous one; and that, though there has been a very slight increase in the ratio of deaths in the last seven years, not amounting to .5 per 1000, the ratio of births has increased to the extent of 2.20 per 1000; finally, that we have no registration of sickness, which is indispensable in any attempt to estimate the success of medicine in opposing ‘the tendency to death.’

“We confess that we cannot form a high opinion either of Dr. Elam's judgment in the construction of statistics, or of his animus

towards modern medical science, when we find him basing upon extremely vague data conclusions so gratuitous and, as it seems to us, so palpably absurd. Suppose there is an increase in the death-rate of the last seven years—though about this, in our uncertainty as to the exact growth of population, we cannot be sure,—we have to associate it with a high birth-rate, with a most eventful decennium, including a spurious prosperity which suddenly and disastrously collapsed, a war which interrupted our principal industry, causing typhus and other diseases of destitution, an epidemic of cholera, and a prevalence of other fatal epidemics. We think a shrewd physician, before writing disparaging things against his own profession, would have exhausted other explanations of a slightly increased mortality; but Dr. Elam seems eager to fasten the blame upon his own art, and makes light of other explanations in order to come down with more effect upon its shortcomings.

“We are amply conscious of the shortcomings of medical art; but we think it admits of demonstration that it is more successful now than it has ever been; that Dr. Elam can be demonstrated to be wrong when his conclusions are applied to the case of the particular diseases which he specifies; and that for any additional mortality from them there is abundant explanation without making humbling comparisons of present and past modern medical science.

“The cardinal diseases, or groups of disease, Dr. Elam says, kill an increasing proportion of the population; therefore, he argues, medicine is less powerful than it was in the good old days of bleeding and purging and mercury. This is a beautifully simple explanation of a fact that obviously may require a very complex explanation. And we believe it to be as absurd as it is simple. Suppose it to be true that the cardinal diseases are increasingly fatal, we find a principal explanation of that fact in the general conditions of life, in the circumstance of the growth of cities, and the multiplication of persons living in conditions that generate disease, and make its treatment, when generated, exceedingly difficult. Take, for example, the infantile diarrhœa of summer, and the equally fatal infantile bronchitis of winter, in such cities as Liverpool, Glasgow, and Manchester; or take the zymotic mortality of such places. The explanation of the mortality from these diseases, as far as we can judge, is to be found, not in the decline of the science or art of medicine, but in the fact that the multiplication and aggregation of human beings in limited space, and with a most limited and precarious supply of food and clothing, generate in them a diathesis which borders on disease, which tends in its very nature to death, and which it would be unreasonable to expect medical science to cope with. No sensible man ever meant medicine to supersede the use of food and clothing, and the need for a proper supply of air. Dr. Elam talks about ‘all our sanitary regulations;’ but they are a mere drop in the bucket compared with the needs of our

impractically huge populations. Witness the state of Glasgow lately.

“This explanation of the large mortality is quite consistent with a belief that medicine has immensely improved as an art. The part of Dr. Elam’s papers which looks weakest to us is that in which he proceeds to particulars as to the decline of medicine. Does anybody but Dr. Elam seriously believe that the ‘prodigious increase’ in the mortality from bronchitis is to be explained by the infrequency of bleeding now, and the administration of stimulants? We may admit that in both these respects we may have gone to extremes. But can it be seriously maintained that the change of medical practice will explain an increased mortality from bronchitis? The older physicians themselves knew that bronchitis soon depressed the powers of life, and recommended a very careful use of bleeding and an early use of nourishment. Dr. Elam himself would admit that the acute bronchitis which kills is mostly that of children, in which the administration of large quantities of stimulants never has been a fashion, and certainly has been a rarer error than the antiphlogistic practice; and that the bronchitis of adults which kills is more or less chronic—a form of senile degeneration, in fact, in which bleeding or mercury would be absurd, and in which the judicious use of nourishment and even stimulants is palpably beneficial. To believe that the difference of medical practice will explain 2067 deaths in 1838 from bronchitis, and 41,000 in 1866, is as credulous as to believe that these figures give any very exact representation of the comparative amount of bronchitis at the periods.

“Let us take the case of another disease which Dr. Elam tells us has perceptibly increased in fatality—namely, pneumonia. Now here we can be particular in demonstrating Dr. Elam’s explanation to be untenable. It so happens that two physicians have recorded an unprecedentedly successful treatment of pneumonia, in studying which one almost ceases to think of it as a fatal malady. One has reported sixty or seventy and the other about fifty cases with the most trifling mortality. Now what is the conspicuous feature in the treatment of these physicians? Exactly the negative qualities of treatment to which Dr. Elam attributes the growing fatality of disease, and in which, in his opinion, consists the decline of medicine as an art—the absence of bleeding, mercury, and purging. Dr. Elam will ransack the records of the older practice, which he so much admires, in vain for anything comparable to the success of recent physicians in the treatment of acute disease. It would scarcely be too much praise to give to modern medicine to say that it has almost demonstrated that acute simple disease is not fatal, and that the great fatality of diseases that still baffle our art is due more to the imperfection of social and political than of medical science. Dr. Elam’s papers are an illustration of what one often

sees in the writings of moralists and poets, but which is not common in the literature of science or art—a magnification of the dead past at the expense of the laborious present. It would be as reasonable to prefer the stage coach to the railway, as to prefer Dr. Cullen's treatment of pneumonia to Dr. Bennett's. There is nothing more easy than to make a mistake in the import of figures; but, considering the unusually obvious sources of inaccuracy in the vital statistics with which Dr. Elam was dealing, and the obviousness of the improvements in medical art, he has achieved, it seems to us, an amount of error in his conclusions which is surprising."

CHAPTER V.

"SARTOR RESARTUS."

UNDER the heading of "Medicine, Disease, and Death," I recently advanced in the *Lancet* certain propositions respecting the increase of mortality in England, and the influence of the Art of Medicine on disease. It may be that in my desire to be brief, and not to weary the reader with many figures, I failed to make myself very clearly understood. Under any conditions, however, it was not to be expected that these views would pass without challenge. Accordingly they have been investigated and reported upon by two critics. One of them, taking the statistical division of the subject, states that the result of examination has been "thoroughly and completely to satisfy us of their utter failure to establish either one or other of his propositions relative to the decline of medicine as a healing art." The other, taking a more general medical view, concludes that I have "achieved an amount of error in (my) conclusions, which is surprising." Both pronounce my opinions "absurd," with such unanimity and persistence of iteration as might appear monotonous to those not interested in the anthology of eriticism.

Perhaps after this I ought to retire from the question, rebuked and convinced. But the subject appears to me,—and judging by the many communications I have received, to great numbers of the profession also,—to be of too much interest and importance to be dismissed by an *ex cathedrâ* verdict, however authoritative; and, therefore, I now take an opportunity of again briefly stating my conclusions, and examining the grounds on which they have been summarily rejected.

Before doing this, it seems necessary to enter into some definition of terms. The critic, of July 31st, complains that my “attractive title”* consists of “three great vague words.” Lest their magnitude and vagueness should puzzle any future commentator, I would state, that Death I understand to mean the cessation of life,—Disease, that abnormal condition of structure or function that generally precedes Death, the proximate cause of Death,—and Medicine, the Art of Healing, checking and preventing Disease,—private and State Medicine.

Disinterred from the mound of commentary, criticism, and obloquy heaped over them, my views are comprised under two perfectly comprehensible heads,—one *statistical*, a mere matter of figures and facts, which no amount of dialectics will materially affect,—the other *rational*, or (if preferred) *conjectural*, in which I attempt to give a probable or possible explanation of these figures; and which must always remain a matter for discussion and difference of opinion. It is well to bear this distinction in mind, as the two have been mixed up in the criticisms with laughable inconsequence.

When I demonstrate by figures that there has been, within the last twenty years, an enormous increase in the fatality of pulmonary affections, it can scarcely be considered an exhaustively satisfactory answer, to say that the “negative treatment” of pneumonia has been so successful, that “one almost ceases to think of it as a fatal disease,” or that “modern medicine has almost demonstrated that acute simple disease is not fatal.” (See *Lancet*, of July 31st). It is rather to the prevalence of such “expectant” ideas as these that I should attribute some part of the increased mortality in question. To the subject

* The whole passage runs thus :—“It is very easy to put together three great vague words, such as give the attractive title to Dr. Elam’s papers; and with the help of statistics, come to almost any conclusion on the subject, to which the mood or fancy of the writer may incline him.” Within certain limits, this is indisputable. It is *easy* to arrive at the conclusion that the sum of the three angles of a triangle amounts to more, or less, than two right angles; or that the sum of the squares of the base and perpendicular is exactly twice as much as the square of the hypotenuse; but how far such reasoning would throw any discredit upon geometry is scarcely doubtful. So with statistics :—their truth is absolute *in their own department*, that is, so far as they are correctly compiled. Easy as it is to “come to *any* conclusion,” I question whether the writer now quoted could so combine authentic figures as to show any possible diminution of mortality during the period under discussion.

of pneumonia I shall have occasion to refer more fully hereafter.

1. My first formal proposition was, that the average death-rate is gradually increasing. The reasons given for rejecting this conclusion are as follows:—That town mortality is greater than that of the country, and that town populations increase more rapidly; that the mortality of the decennium 1851-61 was less than that of 1841-51; that in some of the registration districts there is a decrease of mortality; that the mortality of Salisbury has decreased as much as 7 in the 1000; finally, that statistics are uncertain in their revelations.

With all deference I would suggest that this seems like trifling with an important question. I have taken the entire results of a large kingdom for thirty years on which to found a calculation, and the critic answers that “there are towns like Salisbury where the mortality has been reduced as much as seven per 1000; but we, at any rate, should not venture to claim for medical skill in Salisbury that it has made such rapid progress as to account for so great a reduction in the mortality.” Nor should I. Here is an instance of the error of mixing up facts with the attempted explanations of them. But under any view, these arguments tell nothing against the proposition. It is not because there is an average of four or five per 1000 more deaths in London than in Lewisham; or that during last week* there were in Manchester and Glasgow nine deaths per 1000 in excess of those in London, that I should rank the medical skill in those places in that order and ratio. No “rational being” (to quote the critic) would do so. It is not because in any one year the mortality is less or more than the preceding one that I judge of increased or diminished mortality, nor because one district is lower and another higher in its death-rate. I can point to registration districts that have been so decimated by an epidemic in one given year that the death-rate has been unnaturally low for several subsequent years. It is because throughout the whole kingdom, including every town and country district, taking successive groups of years, there appears a gradual increase of the death-rate, that I infer there is something defective either in preventive or curative medicine.

In reasoning from the two decennial periods mentioned, it

* Written Aug. 1869.

ought to be remarked that between 1841 and 1851 is included the terrible year 1849, in which the mortality of the entire kingdom was nearly two per 1000 higher than any other year since registration began; whilst in the period 1851-61 there was the year 1856, in which the mortality was considerably lower than in any other year. Were these two years eliminated, the result would be very different.

My calculations were based upon the entire averages for thirty years, and upon these I found an increase of nearly one per 1000 in the death-rate. I am content, however, to accept the correction of the critic, who reduces it to $\cdot 5$ per 1000; or even, for the sake of argument, the dictum of the Registrar General, that “the rate of death is, under the same conditions, over a series of years, nearly constant.” What does this imply?

These, amongst other things, are implied in this authorized conclusion:—That out of all children that are born, one-fourth die under five years of age; that of every five deaths, two are children under five years; and that there are, in round numbers, about 100,000 deaths per annum due to what are considered preventible or removable causes.*

2. My second proposition was, that men die now at an earlier age than they did thirty years back. The critic objects that he has “no evidence” that I have gone into the “calculations which alone would warrant” such a conclusion; and alleges that I have “relied exclusively upon the mean age at death.” Curiously enough for the accuracy of this remark, I never alluded, directly or indirectly, to the mean age at death; I knew that process to be fallacious. My assertion was founded upon the undeniable fact, that from the year 1847 to 1861 there was a gradual decrease in the proportional numbers of those who died at advanced ages, that is, above 70; and also upon another most significant fact, for brevity’s sake not introduced before,—that from 1841 to 1861 there has been a gradual decrease in the numbers of *those living* at such ages. Corrected for increase of population, there is a deficiency of above 14,000 persons above 70,—and of 18,000 above 80, comparing 1861 with 1841. Without doubt this can be explained away, but it will be difficult to get rid of the next fact; that above these ages, there is an *absolute* decrease

* See the Registrar General’s Tenth Annual Report for much information on this point.

in numbers, without any correction for increase of population. Between 90 and 95 the numbers living fell off between 1841-61 above 5 per cent.,—between 95 and 100, about 14 per cent., and 20 per cent. above these ages. A reference to the Census Reports will verify all those figures, without loading these pages with more tables.

In further commenting upon this part of the subject, the critic of July 17th has a passage in which the mystification is so profound that I beg leave to reproduce it here. He says :—

“ Without admitting for a moment that rates of mortality give any necessary or reliable indication of disease treatment, we nevertheless subjoin the rates per 1000 of males at different ages relatively to the population in each of the three Census years—1841, 1851, and 1861 : taking those years for the simple reason that they are the only ones in which we have any absolute knowledge of the distribution of ages amongst the living.

		1841.		1851.		1861.
All ages	...	22·38	...	22·76	...	22·68
0—5	...	68·43	...	72·98	...	71·76
5—10	...	9·56	...	8·69	...	6·74
10—15	...	5·10	...	4·91	...	4·33
15—25	...	8·11	...	7·76	..	7·28
25—35	...	9·78	...	9·48	...	9·23
35—45	...	12·17	...	12·36	...	12·65
45—55	...	17·85	...	17·87	...	16·90
55—65	...	31·37	...	30·31	...	30·08
65—75	...	64·82	...	63·96	...	68·90
75—85	...	142·66	...	140·55	...	146·54
85—95	...	296·50	...	282·45	...	310·92
95 and upwards	}	431·64	...	419·37	...	448·35

These authentic statements of fact tell their own story. We may fairly point out, however, that, according to Dr. Elam's own reasoning, we may claim for Medicine, that although she may not have succeeded in reducing the mortality of infants and of sexagenarians, yet she appears to have had a triumph among the great bulk of the population between 5 and 60 years of age.”

What “story” is it that is told by this table? And what possible “triumph” can be claimed for Medicine from its revelations? The truth is these figures seem to have been

copied in happy unconsciousness of their significance. They have no reference whatever, as the writer appears to suppose, to the rate of death “relatively to the entire population ;” but only to the proportion of those dying out of each 1000 then living at the respective ages. Thus, in 1861, between the ages of 85 and 95, there were 310 deaths of males per 1000 ; and between 55 and 65 only 30 per 1000. But these proportions refer *not* to the population, but to the numbers then living at the respective ages. The *actual numbers* of males that died were 17,481 between 55 and 65 ; and only 3912 between 85 and 95. The method of extracting from this “ a triumph” for medicine, is certainly obscure. Read in this way, and bearing in mind the relatively and absolutely diminished numbers *living* at the more advanced ages in the successive epochs, the table most strongly confirms my position. Read in any other manner, it is simply without meaning.

(The following passage was not in the original paper.)

[One other instance of the competence of the same writer to deal with figures, and to criticise others, I here subjoin. Writing on the probable increase of disease, he says :—“ Since the above was written, Dr. Ballard’s Annual Report on the Health of the Parish of St. Mary, Islington, during the year 1868, has come to hand ; and as this very able health officer has for some years kept records of the cases of sickness arising in public practice within his parish, we may mention two or three facts taken from his Report which bear out what we have said in regard to the relation between greater or less prevalence of disease and a rise or fall in the death-rate. Thus in 1867 the public medical officers treated 34,692 new cases of disease, whereas in 1868 they were called upon to treat 40,302 of such new cases ; the general death-rate of the parish being 19·9 per 1000 in the former and 20·7 per 1000 in the latter year. That is to say, the cases of sickness increased 16 per cent. in the year, and the deaths 9 per cent. !! (*sic.*) Making full allowance for the fact that the deaths occurred amongst all classes of the Islington population, whereas the sickness only of the poor is taken into consideration, it yet appears to us that *the facts do show an increased amount of disease which would more than account for the rise in the mortality of the parish.* Dr. Philipson’s Sickness Returns for Newcastle, those of the Manchester and Salford Sanitary Association, and of other health officers in this country, may also be appealed to

in disproof of Dr. Elam's statement* that disease has not increased."—(The *Lancet*, July 17th). Probably it would be difficult to find anywhere a more complete and incomprehensible arithmetical *fiasco* than this. The difference between 19·9 per 1000 and 20·7 per 1000 is not quite *one hundredth part* of nine per cent.; and even granting this marvellous calculation to be correct, we should still be at a loss to know how an increase of sixteen per cent. in cases of sickness should "more than account" for an increase of mortality, amounting to nine per cent. Surely nine persons do not die out of every sixteen who are ill! Yet the writer of these passages advises me to put myself "through a course of study of the Reports of our chief vital statist."!!]

But indeed the cacoëthes of advising is so strong in both my critics, that it quite leads them to overstep the bounds of discretion. The writer of July 31st commends to my attention Dr. Alison's remarks on the value of "one carefully observed fact or case in medicine,"—advice most excellent in itself; not especially original or striking, but still wise, and about as applicable to the subject under discussion as an admonition to study ornithology would have been. The perspicacity of the writer should have enabled him to see that I was dealing with a question of numbers and masses, upon which no "single fact or case," however "well observed," could throw any light whatever.

On this point there seems to have been a curious misapprehension throughout in the minds of both commentators. My primary object was to account for an ascertained increase of mortality, as evidenced by statistics; and in order fully to accomplish this, I discussed the relations of the medical art to disease. The critics seem to take it for granted, on the contrary, that I primarily hypothesized a decline in the efficiency of medicine, and attempted to prove this by statistics. I commence with a statistical question, but they dispute my right to appeal to statistics at all! The writer also of July 31st considers that the "Returns of the Registrar-General are still composed of too vague material," to be of much service in such a question. To such a statement

* I have nowhere made any such statement. I say we have *no proof* that it has done so, which is amply borne out by this writer himself.

in such connection, I candidly confess I have no reply to make.

3. The greatly increased fatality of certain forms of disease, as bronehitis, and affections of the heart and brain, is not and cannot be questioned ; instead of this, my honesty in the representation of the facts is impugned, as in the following passage :—“ We must dismiss with few words Dr. Elam’s endeavour to trace the effects of unsuccessful treatment in the increased mortality from certain diseases. He takes bronchitis as an example. He says that the proportion of deaths to each million of population rose from 135 in 1838, to 1968 in 1866 ; and gives the absolute deaths from this cause in certain years, so selected as to make it appear that the increase was steady and continuous.” For what purpose so meaningless an accusation is brought against me it is difficult to imagine. If I have done so,—if I have “selected” years so as to make appear anything which is not, or in any way garbled or misrepresented facts for my own purposes, it is simply a disreputable fraud, and should be exposed ; but such an allegation ought to be either substantiated or withdrawn. Here is the whole case as regards bronchitis.

From 1838 to 1841 inclusive, the whole average yearly deaths, *registered* as bronehitis, amounted to 2135. From that time to 1847 there is some break in the Reports. From 1847 these became uniform, and are therefore easily compared. There are now full Reports for the 21 years from 1847 to 1867 inclusive, and these are the results, divided into successive triennial periods.

Years.		Total Deaths.		Average of 3 years.
1847-9	...	45·797	...	15·265
1850-2	...	48·975	...	16·325
1853-5	...	69·635	...	23·211
1856-8	...	76·209	...	25·403
1859-61	...	89·331	...	29·777
1862-4	...	103·520	...	34·506
1865-7	...	118·135	.	39·378

I leave the reader to judge from this where is the *suppressio veri*, or the misrepresentation. I have no quarrel with any one for supposing that two and two make three or five, or any other odd number. I only ask for personal liberty to consider the sum to be four.

Now this vast increase in figures does not accurately

represent the real increase in the fatality of that disease. As I pointed out fully in my first paper, part of it is due to difference in registration; cases which were formerly represented as phthisis being now registered as bronchitis. It is easy, however, to eliminate this source of error by adding together the numbers dying from the two causes and comparing the results at distant periods. Taking in this manner the three years 1850-2, and comparing them with the three years 1864-6, we shall find that there is an increase in the annual average of deaths of 816 in every million persons living.*

I do not know that anything would be gained by the further accumulation of figures. So far, it does not appear that I have "achieved" any very surprising "amount of error." One other illustration, however, I may add from a duly authorized source.

Dr. Farr, in his letter included in the twenty-ninth Annual Report of the Registrar General, takes a similar method of computation to that adopted here, with nearly identical results. He deals with successive quinquennial periods, and notices the marked increase in fatality. He says:—"Taking the sum of the mortality from phthisis and all the respiratory order of diseases, *to avoid any ambiguity*, the mortality for the same years was 5·580,—5·803,—5·976,—6·331 (per thousand living). *The cause of this increase deserves to be carefully studied.*" Dr. Farr traces *some part* of it to "dusty particles," to the "smoke of towns," the "shut-up life in chambers," &c., &c. In all this I fully coincide. It was impossible, in the space allotted to me, to give an exhaustive treatise on the "Causes of Death." If any one doubts the vastness of the importance that I attach to town life, and to industrial and social conditions generally, as sources of disease, I would ask permission to refer him to my essay on "Degenerations in Man," which forms the second chapter of "A Physician's Problems." At the same time I have ventured here to suggest, that *possibly* a decline in thera-

* A marginal note, in my returned MS., by one of the critics, pronounces this increase to be "significantly small." I cannot conjecture the mental condition which could originate such a verdict. The increase corresponds to *nearly* one death to each thousand persons living; that being the whole amount of increased mortality from all causes, in the period under discussion. In view of this fact, I should be inclined to consider the comment as "significantly feeble."

peutics *may have* some small share in the result which we have been investigating.

In any case the numerical facts remain; and the corollary, as I originally stated it, is almost syllogistic in its certainty. Death has increased;—disease is the proximate cause of death;—therefore either disease has greatly increased, or medicine (see the definition) is less powerful to check it than formerly. But if disease has increased to such an extent as to cause this augmented fatality, it seems somewhat strange that no attention has ever been directed to the fact, as an illustration of the increasing inefficiency of our preventive measures.

Into the extensive question of treatment, my present limits do not permit me to enter at length. [I shall refer again to this subject in the concluding chapter.] Suffice it to say in this place, that my critics quite misunderstand me when they represent me as wishing to employ bleeding and mercury in bronchitis. My remarks were general, and intended to convey my belief that along with the vast increase in our scientific knowledge of medicine, we had forgotten some things of a practical nature, which might have been remembered with advantage. I have as yet seen no valid reason for relinquishing this opinion.*

CHAPTER VI.

“THE AIMS OF MODERN MEDICINE.”

I HAVE mentioned in the introductory chapter, that an essay bearing the above title appeared in the *Quarterly Review* for April, 1869. It is formally and ostensibly a review of the “System of Medicine,” edited by Dr. Russell Reynolds; in reality it is a sketch of the progress of medical science and art during the last thirty years, written very ably, and in the most candid spirit. To the practical enquirer its most interesting feature is the contrast drawn between the pathological and therapeutic views of medicine, as set forth in the “Cyclopædia” and the “Library” of Medicine, with those now held. From this it appears, as I have repeatedly observed, that there has

* A few sentences more concluded the paper *sent* to the *Lancet*. As they are introduced at the end of the introductory chapter or preface, I have not reproduced them here.

been a prodigious advance in our knowledge and appreciation of disease.

The "Address in Medicine," delivered by Sir William Jenner before the British Medical Association in July last, has a similar tendency, but enters more into the details of practice, as would naturally be expected from a practical physician of such eminence. If I cannot take the same sanguine view of the progress of *practical* medicine as is held by both these authorities, I can at least appreciate and sympathize with the prophetic spirit, which sees the harvest in the seed-time, and translates the future into present fruition.

That the inquiry upon which I have entered is not a work of supererogation, nor a gratuitous reflection upon our profession, I call upon these writers to testify. The author of the Essay in the *Quarterly* says:—

"In truth it is an anxious question for patients, and also a serious one as respects the estimation in which the profession as a whole deserves to be held, whether medicine is walking hand in hand in progress with the sister arts of utility. Has it advanced since the time when those who now claim to teach were pupils? Is what is taught better than the last two collections of similar labours, published a quarter of a century ago, under the titles of the 'Library of Practical Medicine,' and the 'Cyclopædia of Practical Medicine?' It is certainly different, there is no doubt about that; and our first step towards an answer should be the seeing clearly in what the difference consists."—*Quarterly Review*, July, 1869, p. 536.

In the "Address," Sir William Jenner observes also:—

"Even the most hopefully minded must now and then be tempted to doubt if medicine be really advancing as a practical art."—*British Medical Journal*, July, 1851-69, p. 114.

Seeing, then, that no apology can be required for pursuing this investigation, I shall proceed to inquire how far the alleged advances are such as to *power*, or to *knowledge* merely. In noticing some of the most striking instances, selected from the "Aims," &c., I shall not attempt to follow any system, but merely comment upon certain passages as they occur.

The first noteworthy passage relates to the difference of our comprehension of the nature of tubercle now and in past times. In illustration, the writer quotes "from a first-rate article on pulmonary consumption (in the Cyclopædia) a sug-

gestion that emetics may be useful in the earlier stages, to uproot the tubercles from the tissue in which they are imbedded, and thus to cure the disease. It reads like a caricature.” (p. 540.) Considered as a matter of knowledge, it certainly does; though even now I do not think we have arrived at the “finality,” or “rest and be thankful” stage, in our knowledge of the pathology of tubercle. But what of it as a practical matter? Are we to consider that observation is a product of this generation alone, and that the benefits formerly supposed to follow the use of certain remedial agencies were merely imaginary? Emetics would assuredly not uproot tubercle from the tissues; but would they be of no service in clearing periodically the bronchial tubes from excessive secretions, and so obviating the necessity for the perpetual wear and tear of the cough? And have we certainly improved our practice by altogether neglecting them?

Sir William Jenner’s observations upon tubercle and phthisis are very valuable, as enabling us to bring the practical question to a distinct and clear issue. He states that we have “gained certain knowledge of the means of preventing, to a great extent the occurrence, in a large number of cases, of the most fatal of chronic ailments,” *i.e.*, phthisis. He also notices the improvement in our treatment of all tubercular diseases,* by means of cod-liver oil. (Loc. cit. p. 118.) Now if our preventive measures are more efficient, and our curative

* In both papers much stress is laid upon the advantages of oily and fatty remedies in phthisis—a class of medicines “not alluded to either in the ‘Cyclopædia’ or the ‘Library.’” Perhaps, *per contra*, it is not uninteresting to notice that in one of our most recent, large, and professedly exhaustive treatises on the Principles and Practice of Medicine, there is *but one* remedy even alluded to for the disease in question, and that is cod liver oil. No doubt it is a most valuable addition to our *materia medica*, but it can scarcely be considered so entirely all-sufficient as to set aside the necessity for mentioning, however slightly, any other. Surely, if even for the sake of rejection or reprobation, there were some other agents worthy of notice, as iron, iodine, or the hypophosphites. This is one of the “fashions” of medicine. The result of my own experience in the treatment of phthisis, in a district where the disease is exceptionally prevalent, is, that much more can be done, if not for cure, yet certainly for amelioration of suffering, and for prolongation of life, by directing every effort to *keep down fever*, and by meeting symptoms as they arise, rather than by any one uniform plan of specific treatment.

I may add also that the treatment by oily and fatty remedies, even if mentioned neither in the “Cyclopædia” nor the “Library,” is not so great a novelty as here intimated. Thirty years ago, the most popular of all remedies for phthisis and other pulmonary disorders was a diet of milk in which suet had been boiled.

treatment more successful, it follows as an inevitable corollary that the deaths from such causes must be diminished. How does this conclusion bear the test of figures?

In reckoning the mortality from these causes, Dr. Farr recommends* in order to "avoid any ambiguity" (Letter 1866), to add together the numbers dying from "tubercular diseases," and from "diseases of the respiratory organs."

Adopting this course, and taking the average of three years in each case, we find that the deaths from these causes, between 1851 and 1853, were 6581 to every million persons living; whilst between 1865 and 1867 they amounted to 6903 per million. Thus, simultaneously with an undoubted advance in abstract knowledge, and with our alleged improvement both in preventive and curative medicine, the aggregate result is unfavourable.

The writer in the *Quarterly* lays much stress upon the consideration that our views as to disease in general are now more philosophical. "Disease daily more and more presents itself, not as an addition to life, but as something less than life. The life of the individual . . . is the point for the physician's consideration, and to supply its deficiencies is his chief aim." (p. 541.) As a general statement, this view of disease will not bear strict investigation, inasmuch as it will not include all the phenomena. As a guide to treatment, I believe it to be impracticable at best, and, if attempted to be practised, worse than useless. It is the foundation of what is called "Restorative Medicine," and so far as the end and aim of the system is to "restore" health, it is good but not new. So far as it professes to remove all removable disease by "supporting the strength of the patient" (see p. 544), it may be new, but is not good, nor, I may say, consistent. For in the "Restorative Pharmacopœia" I find not only "restoratives," but also "arrestants" and "destructives,"—new names certainly, but in their essence not differing materially from what we used to call "food" and "medicine." "Restoratives" proper include food and some tonics; "arresters" may be considered as representing alcoholic stimulants, opium, and some other drugs; whilst "destructives" are but our old friends, purgatives, sudorifics, diuretics, expectorants, mercurials, counter-irritants, &c., &c., under a new generic name. Except as a matter of

* See p. 44, *ante*.

terminology, I cannot see in what this differs from the much-despised “Ecclecticism,” which in my opinion ought to be the beginning and end of all medical art. I mean that intelligent eclecticism which considers each case as a *special* departure from proper co-ordination of the functions, and *selects* its mode of treatment accordingly, not hypotheating either an excess or a deficiency of life for all cases alike, and so acting upon preconceived theory. The special illustrations of this point I defer until the next chapter.

There are two other alterations in our views of special diseases, mentioned in the Essay, that I think are changes, but not *advances* and improvements. I refer to scarlatina and whooping cough. The writer seems to congratulate the profession on having lost the “idea of a *materies morbi*” in the former case; which seems to me a determined setting aside of facts for theory. In reference to whooping cough, I have no hesitation in expressing my conviction, founded on recent careful investigations, that Laennec’s views of its nature were more accurate than any that have succeeded them, and that it is really a specific bronchitis, the nervous complications being merely secondary. The writer of the “Aims” very wisely and properly rejects with some good-humoured derision the idea of a “change of type” in disease; and considers that “the change of type is in the doctor, not in the diseases or the patients” (p. 551). His comments upon the evils of “stimulation” and “expectancy” are so excellent that they deserve to be reproduced in letters of gold, especially this sentence (p. 557) :

“ASCLEPEIADES VERY PROPERLY CALLED ALL MEDICAL SCIENCE WHICH DOES NOT END IN ACTION, ‘A MEDITATION UPON DEATH.’”

Sir William Jenner, in his earnest and admirable “Address,” proposes formally to “survey the progress which medicine has made as a practical art in our own time,” and believes “that having regard to the attainment of its practical aims and objects *as an art*, no science has advanced more during the period in question than has the science of medicine. *As an art*, I say, for while medicine is universally admitted to have advanced as a science, its progress as an art is frequently regarded as trifling, and often denied.” (*Loc. cit.*, p. 115).

Here is the whole question brought fairly to an issue, and it may be reasonably supposed that (making allowances for the limitations of time) from such an authority, and on such an occasion, all would be said that was possible on the subject. I propose then to examine *seriatim*, but as briefly as may be, the instances adduced of advances in the Medical *Art*.

The first "great advance" in the Medical Art is mentioned as the "separation of chronic degenerations from diseases" (p. 115), illustrated by our knowledge of "retrograde metamorphosis," "diminution of elasticity" in advancing age, "rotting petrification," "fatty metamorphosis" of the muscular tissue of the heart, as distinguished from "atrophy proper," and a clearer "recognition" of the true pathology of various other changes of tissue. We are also enabled "clinically to distinguish" certain conditions of organs from certain others, and to "attach their true significance" to some other "secondary changes," &c., &c. All these are assuredly most valuable additions to Medical *Science*. We have knowledge, recognition, appreciation, perception of new relations, and understanding of phenomena, to an extent scarcely to be over-estimated. I think it will be acknowledged, however, that this is still science rather than art, and that the therapeutic results are hitherto very doubtful and obscure.

Applying the previous remarks to clinical study of diseases of the heart, the author concludes that—

"Structural changes in the valves of the heart are referrible to one of three classes: imperfection in development, acute endocarditis, degenerative changes. And yet further advance of clinical knowledge has shown that non-fatal acute endocarditis is almost limited to acute rheumatism, and that degenerative changes, sufficient in degree to interfere with function, do not occur in the valves of the heart till middle life, and rarely till advancing middle life" (p. 116).

I quote this passage for a purpose which I feel some diffidence in following out, as I feel compelled to say that it is entirely at variance with my own experience, and, therefore, our "advance" in knowledge in this respect seems doubtful, to say the least. In my hospital practice over a space of twelve years, a very considerable proportion of the cases of endocarditis that occurred, at least one-half, were absolutely

without any rheumatic history whatever; and I have seldom been a year without seeing three or four instances of this idiopathic affection in children from seven to twelve years of age, in many of which instances the nature of the affection was verified by *post mortem* examination after prolonged chronic disease.

Sir W. Jenner then proceeds to instance the value of the thermometer in diagnosis and prognosis, and subsequently our “appreciation of the influences of various mechanical consequences of primary diseases,” and our improved knowledge of the conditions attendant upon blood-poisoning. Still *science*,—the art not materially altered.

Further: “Advances of knowledge which have followed are manifested, 1. By the more correct appreciation of the relation between objective signs and the lesions found after death. 2. By the separation of diseases previously confounded as one. 3. By the discovery of diseases formerly unknown.” All exceedingly interesting, and not without its prospective utility, but certainly, at the present time, appertaining still to science rather than to the therapeutic art.

The author next enumerates the services rendered to medicine by the microscope, the thermometer, the laryngoscope, the ophthalmoscope, the sphygmograph, and the balance, in all of which I most cordially acquiesce. He next proceeds to classify the “aims and objects of medicine” under these four heads—

1. To prevent disease,
2. To cure disease,
3. To prolong life, and
4. To alleviate physical suffering.

Under the first division, it is shown how great and powerful an agent in the spread of cholera and fever impure water is; and improved drainage is recommended as a preventive of phthisis. Our improved treatment of fever is also commended. The remarks that have before been made as to phthisis will apply equally to fever. If our preventive measures are more efficient, and our treatment better, it ought necessarily to follow that the fatality from such a disease should be diminished. Now how stands the case really? The average fatality of fever for the three years 1850-2 was 954 for every million persons living; whilst for the three years 1865-7 it was 969 per million; the mean for the whole 18 years being 923.

It may be that these figures indicate merely that we do not act up to our knowledge ; and this is the very point that I am striving to illustrate. So far, at least, it would appear that our advances are those of knowledge and appreciation, rather than of therapeutic utility.

There are, however, some instances of true *discovery* in therapeutics ; and these are well worthy of mature consideration ; inasmuch as they appear to me to indicate the direction that our investigations ought to take, and the mistake we have been making of late years, in neglecting the specific properties of drugs, for theories of nutrition and restoration. There are two diseases which we unquestionably treat better now than we did twenty years ago :—these are epilepsy and dysentery. We know with almost a certainty that the fits of epilepsy can be marvellously controlled by bromide of potassium ; we also know that the treatment of dysentery by ipecacuanha has been the means of saving multitudes of lives that would have been lost under the old plan. Now it is worthy of note that neither of these discoveries have any relation whatever to any theory of disease, or in fact to science in any wise, properly so called ; both are simply empirical, that is, the result of experiment. It is not because the bromide is a tonic or a sedative, or belongs to any remedial class, that it has been found useful in Epilepsy. It may have these properties ; but the knowledge of them is only secondary to its application with success. The same may be said of ipecacuanha : it is simply a matter of experience that it cures dysentery ; and not because it has been known for many years to be emetic or sudorific.

For some time back it has been the fashion to repudiate the idea of “ specific ” medicines, perhaps partly because their use necessitates the laying aside of theory, to which our age is so much addicted. Whatever the cause, I believe the fact has acted unfavourably upon the progress of medicine in two ways ;—first by preventing the discovery of new experimental methods of healing ; and secondly, in one instance at least, by causing us to relinquish one long and well established mode of cure of a certain affection, which had until recently been supposed to be directly amenable to a certain remedy. I refer to syphilis and mercury. I would not dogmatize nor rashly assert anything on this subject ; but would merely state this

fact. Since the doctrine that mercury not only did not cure, but was injurious to syphilis, began to be widely disseminated, the mortality due to this disease has increased from a yearly average of 35 to one of 80 per million persons living. This is a pregnant fact, and one that I prefer to leave without comment.

Besides the instances already mentioned, Sir W. Jenner enumerates these “strides made in drug therapeutics;”—(1) the influence of “iron on the cachexia of the aged; (2) of digitalis as a cardiac tonic; . . . (3) of sulphites and sulphurous acid, and of carbonic acid, in the treatment of vegetable parasites; and (4) of Faradisation and the continuous current in some morbid states of the nervous system.” (p. 118.) All these I believe have their great value; but the first can scarcely be considered a *discovery* of modern times. The third is comparatively new, and good in its results. As to Nos. 2 and 4, the respective diseases have increased so marvellously in fatality during the last 25 years, that we can scarcely claim to have arrived at any materially improved methods of prevention or cure. (See chap. I.)

Thus, on a candid review of our progress, the general conclusion at which we are compelled to arrive is this;—that whilst our abstract and scientific knowledge has most wonderfully increased and developed, our therapeutic methods, judging by aggregate results, have not benefited to any corresponding extent; and those improvements which have undoubtedly been effected, have been arrived at by a process usually reproached as unscientific,—that is, by the discovery of specific agencies by means of experience, rather than by the application of any previously ascertained scientific principles.

There is also some reason to doubt whether, by our attempts to formulate our knowledge too precisely, and to reduce it to simple comprehensive expressions, we have not in some cases been led to receive general axioms as conclusive and certain, which would not bear the test of practical experience. Such an instance I have ventured to suggest above, in reference to heart disease; two others are, I believe, contained in the following passage from the “Address.” (*Loc. cit.*, p. 118.)

“Means were formerly sought to strangle a fever, to cut short a pneumonia. Increase of knowledge has taught us that these diseases always terminate within a limited

period, but are *never cut short*; while collections of facts have proved what, in the present state of pathological and physiological knowledge, might have been predicated; viz., that a larger proportion of these diseases terminate in health under restorative treatment than under depleting remedies."

As to fever, my experience does not teach me that it "*always* terminates within a limited period." One of my most recent cases retained the special and peculiar characteristics of typhoid fever for sixteen weeks, and then recovered; and this is by no means a solitary case. But there is, at the present time, an almost irresistible tendency to reduce the phenomena of disease to a Procrustean formula. If disease *A* accords with what has been "predicated" of it by formula *Ax*, then it is disease *A*. Should it venture to fall short of, or to overstep this formula, it is *not* disease *A*, and must not be discussed along with it. Thus typhoid fever which lasts less or more than the prescribed time, is not typhoid fever,—by the exigencies of the formula. And again, if it is "cut short," it equally is not the disease in question.

These remarks appear to me to apply with especial force to pneumonia. I do not usually see pneumonia terminate at any defined time, and assuredly I do see it "cut short," not unfrequently, by appropriate treatment. But the answer is conclusive and irrefragable:—"These cases are not pneumonia;" inasmuch as they do not fit in to the formula.

It has often been a matter of surprise to me, where writers on such subjects can find a sufficient number of cases wherefrom to deduce such absolute and incontrovertible dogmas. Pure pneumonia has always appeared to me to be a comparatively rare disease, at all events in the adult. In the year 1867 there were 21,118 deaths recorded from pneumonia. Of these 13,734 were children under five years of age. It is well known that in young children the absolute and physical diagnosis of pneumonia, from bronchitis, congestion, or acute tuberculosis, is very difficult; and perhaps the question at issue applies more to adults than to children. Leaving these last out of the question, there were in that year 7384 fatal cases of pneumonia above five years of age, giving about one fatal case to each two medical men throughout the kingdom. It does not appear to me that the whole scope of these

phenomena gives sufficient warrant for any one to conclude that such diseases are “never cut short.” Many men think they are; and it is proverbially difficult to prove a negative. If when a case appears to be cut short, it is *therefore* pronounced not to have been pneumonia, there can be no answer to such a verdict, save assertion and counter-assertion; otherwise, I should be slow to reject the cumulative evidence in favour of an effective and *curative* treatment of pneumonia, but certainly by measures widely differing from the “supporting” or “restorative” system of treatment.

I have dwelt more fully on this subject because it is to the prevalence of these ideas that I attribute some part of our present languor and indifference to therapeutics. If diseases will recover without artificial intervention, it is considered unwise to interfere. Truly all disease is not fatal of itself, and a considerable proportion would recover if left alone, or with only judicious nursing. But I am convinced that by *properly directed* medical, yea, even drug, treatment, this proportion might be greatly increased, the greater comfort of the patient assured, and the after consequences of the disease in great measure obviated.

CHAPTER VII.

ECLECTIC MEDICINE.

SOME years ago, Dr. T. K. Chambers gave to the profession a singularly interesting and suggestive work, entitled “The Renewal of Life,” starting with a theory of disease to which I have many and serious objections, but illustrating that theory by a practice so enlightened, so comprehensive, and in fact so eclectic, that it was at once manifest how much, in medicine as often in theology, the man is greater than the creed. The text of the book is as follows:—

“DISEASE* is in all cases, not a *positive existence*, but a *negation*; not a new *excess* of action, but a DEFICIENCY; not a *manifestation of life*, but PARTIAL DEATH; and therefore the BUSINESS OF THE PHYSICIAN is, directly or indirectly, not *to take away* material, but to ADD; not

* The marks of emphasis are in the original text.

to *diminish function*, but TO GIVE IT PLAY; not to *weaken life*, but to RENEW LIFE. These are the principles of RESTORATIVE MEDICINE" (p. 15).

Now, as I have before remarked, the object of all curative treatment is the *restoration* of health, absolute or comparative; and in this sense all medicine is "restorative." In any other sense the term seems to be objectionable; first, as being based on an erroneous theory of disease; and secondly, as leading to undesirable methods of practice.

1. The theory that *all* disease must be a *negation* and a *deficiency* seems to me untenable. In the ordinary use of words we can scarcely call a parasitic disease—for instance, such as *scabies*—strictly a negation. An acute eczema does not naturally suggest a deficiency. Again, if the hard, incompressible pulse that accompanies some diseases be an indication of deficiency of function, surely the reverse condition, the small feeble pulse, should, *cæteris paribus*, be held as a sign of increase of function! In the same manner, increase and diminution of sensibility, of mobility, of cerebration generally, cannot practically and *usefully* be classed as all alike evidences of "partial death." And yet again, some diseases seem absolutely to require food and stimulant for their removal; whilst others—to instance only thoracic aneurism—equally demand the withdrawal of nearly all nourishment, with other depleting measures. It is not impossible by a strained use of dialectics to make the one formula include these diverse phenomena; but by this forced application it becomes useless as a theory for the rational comprehension of disease;—a negative evil, but entailing a positive one in the sequel.

2. This theory points to a method of practice, either inconsistent, impracticable, or injurious. If it be the duty of the physician "not to take away material, but to add," it seems to follow naturally from this that to be consistent he must not bleed, nor blister, nor deplete in any way. And yet in the illustrative cases related in the admirable lectures that follow, leeches, venesection, counter-irritation, and low diet (one or other, if not all), are made use of freely in low fever, in rheumatism, in pneumonia, in aneurism, and in various other affections. Doubtless it is explained in each instance how this practice is reconciled to the "restorative" principle;

but it thereby becomes simply eclectic—that is, a choosing from every system of treatment that which appears to be especially adapted to the individual case. And yet the author is somewhat unnecessarily severe upon this eclecticism, which he calls “a mere negation—a denial of the principles as ends, and an adoption of them as means—as *possible* means towards an *unknown* end.” (p. 12.)

I have no claim, nor desire, to enter upon a formal criticism of the very instructive work in question. This brief commentary is but introduced here because I believe the *text* already quoted, and the phrase “Restorative Medicine,” have had a powerful, and by no means salutary influence upon the methods of practice pursued by great numbers of our profession. In the hands of the author, formulas such as these are modes of expression, and aspects of disease and therapeutics, moulded and used with the skill of an accomplished physician. But with the masses they become something else. Disease (say they) is deficiency of function, and this deficiency is debility. We must not take away material, but we must add, *i.e.*, we must use no depletion, but we must give food and stimulant, which will supply the material for increasing the strength. That this is the doctrine deduced from this theory, and that the consequences of it are very widely spread, I am well convinced. It is perfectly true that a writer is not answerable for the errors of interpretation that may attend his views; but it must be remembered that in these pages I am speaking of the present condition of medicine as I believe it to be actually, and not as it ought, rationally and logically, to be.

But the doctrine that in my opinion has most, and most injuriously, affected the practice of the present day, is that which upheld wholesale stimulation as the one great principle of treatment in a vast proportion of even acute cases. The “restorative” theory was, in its essence, and properly understood, good; but it has been misinterpreted and misapplied. The “stimulant” practice was essentially bad, but easy, and has been extensively adopted. Great numbers of our patients now take their medicine with alacrity, who would formerly have thrown it out of the window. I believe that a very large proportion of our practitioners have adopted a compromise between these two systems as their rule of practice; partly out of deference to authority, partly because it is more agreeable

to their patients, and partly because it is easier than a treatment founded upon careful consideration of each case, and an intelligent eclecticism as to remedies.

When in my papers in the *Lancet* I dwelt upon the evils of "expectancy" and "stimulation," and hinted that we now perhaps too much neglected certain forms of active treatment, involving depletion, my critics arrived at the summary conclusion that I recommended bleeding and mercury for bronchitis. Perhaps this was due to my own obscurity of expression in striving after brevity. To do away with this misconception, and to illustrate what I believe to be the prevalent errors of treatment at this time, I think it best in conclusion to give a few cases, very briefly, in which it would appear that practical mistakes originated in the belief that "Disease" meant "Debility," and must be counteracted by "support" and stimulus.

1. W. H., aged 50, was the subject of chronic bronchitis, with which he had been affected for six years. The most distressing symptom at the time I saw him was a violent and long-continued spasmodic cough, which exhausted his strength very rapidly. Some time before this he had had brandy ordered, as his constitutional strength seemed to be much impaired. From certain circumstances I arrived at the conviction that this brandy and the spasm stood in the relation of cause and effect. It was therefore gradually reduced to *one tea-spoonful* at a time, and then to half that amount. The violence of the cough proportionately abated, and absolutely ceased the very day that the stimulant was entirely withdrawn. The original disease was not removed; but for the time the patient was restored to very greatly increased comfort. I mention this case, so meagre in its details, only because it is one out of a great number, so like it that their relation would be tedious, that led me to form the opinion, formerly expressed, that alcoholic stimulants are generally "poison in bronchitis." Here follows one more.

2. A. M., female, aged 35; of a feeble, small appearance, debilitated by previous organic disease, not connected with the bronchitis, for which she was admitted under my care to the hospital. The face was pale and anxious; the pulse small and compressible, about 90; the breathing short. The stethoscopic signs were those of general bronchitis, extending to the small

tubes. Previous to my visit this patient was put upon good nourishing liquid diet, with some wine; and ammonia for medicine. This treatment appeared so much in accordance with the requirements of the patient, and of the "spirit of the age," that I continued it for three or four days. There was not, however, any improvement; and on the morning of the fifth day the signs of congestion had much increased, and there was some slight pneumonic crepitation at the base of the right lung. Upon this, notwithstanding her apparent debility, I withdrew all stimulant, somewhat lowered the diet, and ordered Liq. Ammon. Acetat. with Ipecacuanha as medicine. The case was watched with some anxiety, but the result justified the rather unusual measures adopted. In twenty-four hours there was very marked relief, and by the morning of the seventh day there was no sign of pneumonia, the general symptoms were much ameliorated, and the recovery was progressive and rapid.

In the next case there was an error in diagnosis, and subsequent to that, an adoption of popular routine treatment in the "supporting" and "stimulating" direction, that might have led to most disastrous results.

3. N. P., aged seven, a dusky-complexioned boy, of West Indian birth, the heir to a very large estate, was seized on the 10th of February, with illness. He complained of great pain around his waist, coughed a short distressing cough very frequently, breathed short, and had considerable difficulty in lying down. The medical man who first saw him considered it a case of pneumonia; and being much impressed with the prevalent beliefs in medicine of the day, and further, being persuaded by the mother that the child had always been of a delicate constitution, his great object was to "support" the strength, so that the disease might not wear him out with exhaustion. With this view he ordered beef-tea *ad libitum*, with wine every four hours, milk and eggs, and all the usual catalogue of restoratives. When I first saw him, two days afterwards, there was no amendment, but the distress appeared increased. He was propped up in bed, and could scarcely bear to be touched anywhere. The tongue was loaded, the pulse quick and feeble, the breathing short, and the hacking cough very frequent. I could, however, detect no sign whatever of pneumonia, or any mischief in the chest, by the

stethoscope. But I learned that the mother was weak and indolent, and was firmly impressed with the conviction that her son was of delicate constitution, and must be pampered in all things. She therefore indulged him in every form of improper and indigestible food to which he took a fancy; and when these produced their natural effects, she avoided aperients, lest they should "weaken" him. The case was patent, and the "supporting" treatment was adding fuel to the fire, which would soon have consumed the patient. A smart dose of calomel and scammony, and the substitution of water gruel for wine, &c., with general reduction of diet, very shortly effected a permanent cure.

The two next cases to which I shall allude are cases of heart disease, and may be viewed as companion pictures, mutually illustrative. In the one case fatal mischief followed the ill-timed adoption of "restorative" methods; in the other, a more favourable result accrued from the timely use of depletion. Both were cases in which the affection of the heart was well known and recognised; the only difference of opinion was as to the best methods of prolonging life, and increasing comfort. I shall not give even the most cursory sketch of their history, but merely allude to the especial epoch in which difference of practice came to be discussed.

4. In the summer of the year 1867, I visited Mr. D., aged 37, who had for many years suffered from valvular disease of the heart, following rheumatic fever in early life. A few weeks after my first visit, Dr. A. saw the patient with me, and expressed his opinion that it was impossible he should live until the winter. My own impression was quite different, viz., that if not treated too much, he would live a year or two, perhaps much longer. I only mention this as having a bearing upon after events; he did in fact live until November, 1868. During this time the essential symptoms of the disease did not materially alter. The principal serious interruption to comfort consisted in periodical attacks of sickness, with an immense deposit of lithic acid in the urine. This occurred every four or five weeks, and lasted some days, with great depression of strength, and utter inability to take or to retain food. In the intervals the patient was moderately comfortable, and could occasionally attend to business, to a small extent. During these attacks, it was my custom to withdraw all but

the most simple food, and to discontinue the wine taken at other times, giving some simple alkaline or effervescent medicine. This practice, whether good or bad, was always followed by a tolerably prompt recovery.

After above a year of these alternations, it happened that I was absent from home during one of these "lithic acid" attacks, and Dr. A. saw him alone, for the first time since the previous year. He expressed no surprise on seeing him alive twelve months after the prescribed limits, but pronounced that the practice of withdrawing food and stimulant during these attacks was bad,—that the sickness arose from "Debility," and must be counteracted by food, stimulants and iron, after "settling the stomach" by an opiate in some form. This he proceeded to accomplish by administering a dose of chlorodyne, which was so far successful that the stomach *was* settled; but the patient only survived the shock of the restorative process about three days; decided collapse supervening shortly after the dose of the sedative.

5. Mr. F., ætat. 55, was the subject of serious mitral disease for seven or eight years. There was no rheumatic history. During the autumn of 1863 he had a sudden and most violent attack of dyspnoea; he could not lie down; the walls of the chest were immovable; no air could be detected entering the chest by the stethoscope, and the distress was most painful to witness. When I first saw him, he had been thirty-six hours without any sleep, and was suffering terribly. I suggested to the medical man in attendance some depletion. He, however, implored me, almost pathetically, not to advise such a course; there was organic disease,—there was extreme debility; we should kill our patient, if we "took away his life's blood!" I yielded, on the stipulation that it should only be for a few hours, if no relief had followed the means then employed. In the evening, the distress was even aggravated; the face was becoming dusky; and the pulse feeble and flickering. With some difficulty I accomplished the application of a dozen leeches over the heart. As the blood oozed from the bites, the patient sunk slowly back in bed,—the chest expanded with a series of sighs, and before the last leech had dropped off, he was fast asleep, breathing easily. The recovery was rapid and uninterrupted, so far as the acute attack was concerned.

6. In the summer of 1865, I was requested by a medical friend to see with him Mrs. T., aged 42, who, he informed me, was suffering under hopeless organic disease of the stomach. For six weeks she had not retained one particle of food, liquid or solid, and was dying (so it was stated) of exhaustion. I found her suffering great pain at the epigastrium, with constant nausea. The tenderness was too extreme to permit of any satisfactory local investigation, but there were no very obvious signs of any hardness or enlargement; and the body generally was *well nourished*, not wasted. Although she could retain nothing, the attendants were giving some nourishment, with weak *brandy and water*, at short intervals, "lest the strength should be let down." This ease appeared to me probably to be one of those anomalous affections of the stomach, not dependent on organic disease, which so closely simulate it; and considering its entirely hopeless nature, if the view first entertained were correct, I thought and represented that we could do no harm by treating it on this hypothesis. I suggested therefore the entire withdrawal of *all food* and medicine for a considerable time; and anticipating that the patient would become *hungry*, we ordered that her appetite should be indulged with only a morsel of ice. This treatment appeared shocking to the friends, and even produced some mild remonstrance from my friend, who was slightly imbued with "restorative" views. The plan was, however, carried out; and in forty-eight hours, she was practically well; she was never sick any more; and the ability to take food and the strength returned simultaneously.

Some months after, I saw a similar case, where it was stated that the sickness had lasted seven years. Here the emaciation was extreme, and the sufferings were very great. A modification of the same treatment was adopted, with exceeding relief. It was not so immediately successful, however, as in the former case; the "*habit* of vomiting" was too firmly established; and there were many relapses before final recovery.

7. Mr. P., aged 54, a stout, healthy looking man, had been for three years the subject of eczema. When he consulted me, his legs, thighs, and the lower part of his body were in a dreadful state. His sufferings were so extreme that he longed even for death to relieve him. He had been treated, so far as drug treatment went, in the best manner possible, by

a highly intelligent practitioner, who was, however, impressed with the truth of "restorative" doctrines. In consequence of this, and of some supposed constitutional debility, the patient had been allowed full meat diet, and half a bottle of wine daily. This was gradually reduced, without any change of medical treatment; but it was not until the meat was taken only once a day in small quantities, and the wine cut off entirely, that a cure was effected. In another very similar case, the disease resisted all treatment until the patient was reduced to a very small quantity of white meat, fish or chicken, three times a week, with entire abstinence from stimulus. There is often, however, much difficulty in inducing the patient to submit to such regulations. In one case, that of a clergyman, I convinced him that proper regulation of diet, with appropriate medicine, would effect a cure in three weeks; but on the recurrence of the complaint, to which he had been for years periodically subject, he told me that he knew *three months* of arsenical treatment would cure him, and he preferred adopting this, as my dietary "did not suit his constitution."

Whilst referring to diet, I cannot refrain from mentioning the change that has taken place during late years in the nourishment of infants and children. Formerly milk was considered their proper food; now we see infants a few months old having broth, beef-tea, even meat, with a miscellaneous farrago of "infants' foods," that, I have no hesitation in saying, lay the foundation for interminable mischief in many constitutions, and are quite sufficient to account for many of the frightful eruptive diseases of children, that are now attributed to vaccination by blatant charlatans, who want "a ery." In hospital practice it is a matter of almost daily experience to see children covered with eruptions or scabs, which are attributed to this cause by their misguided parents, who state indignantly that the child "never had a spot on it, until it was vaccinated." On inquiry as to the child's diet, the answer is carelessly given that "it has what we have," namely, bacon for breakfast, and throughout the day a dietary on the same scale; and this under twelve months of age. Too often the child's appetite is permanently depraved by this treatment; but when the mother can be induced to return to simple and natural nutriment, the disease invariably subsides.

Some time since I saw the child of a clergyman, whose

mother was endeavouring to bring it up *without milk*. At the age of eleven months it was nearly the most complete skeleton I ever saw, yet there did not seem to be any definite disease. The mother expressed her surprise at its condition, as she gave it the "*strongest nourishment possible*," but "milk produced spasms," and therefore she had been advised never to give it any! In short the child was dying of inanition, at the same time that food, of a sort, was given it in abundance. It required some slow and careful education to the taking of milk again; but that was really all the treatment it required, and its recovery of strength was rapid and uninterrupted. But this is a digression, and I hasten to return to and conclude my subject.

Cases such as those above related might be multiplied almost *ad infinitum*, but I refrain. My chief aim in these very brief sketches has been to illustrate my belief that in the temples of Medicine has been set up a false worship, a Shamanism, of which "Debility" is the Mumbo-Jumbo, who is to be propitiated by large quantities of (relatively) indigestible food, with wine, or brandy and water, at short intervals. In general, also, my object has been to indicate that our educational systems need reform; that many of our theories may be advantageously set aside; that many of our methods of practice require revision; and, finally, that if we are permanently and effectually to withstand the waves of ignorance and quackery that are for ever beating against our profession, it must be by the substitution for formula of an enlightened, thoughtful, and, in one word, an Eclectic System of Medicine.

THE END.



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